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# THE JOURNAL
## OF
# THE ROYAL ASTRONOMICAL SOCIETY OF CANADA

DEVOTED TO THE ADVANCEMENT OF ASTRONOMY AND ALLIED SCIENCES

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## ROYAL ASTRONOMICAL SOCIETY

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THE GREAT METEORITE BROUGHT BY PEARY FROM NEAR CAPE YORK, GREENLAND

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# METEORITES

BY H. B. COLLIER

LIKE a sentry on his beat, the earth yearly makes his round, and while he pursues his course at seemingly reckless speed he is met with a storm of missiles as from an unseen enemy. The fusilade is never-ceasing, though waning at times to desultory shots and then again waxing into the fury of a battle.

Yet the little earth rolls majestically onward, quite unconcerned with this puny opposition. And like soldiers in an armored train, heedless of the little bullets pattering on the sides, we sit behind our armor of air and fear not the celestial projectiles. Careful calculations show that probably as many as twenty million of these enter the earth's atmosphere every day.

We have reason to feel the utmost confidence in this armor. It has stood the test of centuries successfully. For how many of these missiles think you have succeeded in piercing this shield of air and reaching the earth in recognizable form? So far as we know, the record is about 700. We are undoubtedly well protected. Million after million of meteorites every hour of the

day and night, come speeding towards us only to become dissipated or consumed in our atmosphere, by friction created through their own excessive speed. The ocean floor is covered with a thin layer of the ash.

We find these few hundred objects intensely interesting subjects of study. They bring to us messages of other worlds. They are the envoys of the celestial domains, coming to us laden with gifts to science. We welcome them.

I hold in my hand a meteorite. It is a world. The myth of Atlas supporting the earth becomes in the light of knowledge more of a possibility, a reality. Callous indeed are we, if, as we gaze upon this little world, our minds are not stirred with wonder and conjecture as to its former home, its travels, and its experiences. Could we but open the pages of its history and even partly satisfy our curiosity! But no; the storehouse is locked and we have not the key to the inner chambers. Our scanty knowledge permits us to secure but little.

But a century ago the scientific and even the popular world scoffed at the idea that masses of matter could come from outer space and strike the earth, in other words that stones could possibly fall from the sky. Even to-day there is much misinformation current regarding their character and the conditions of their fall.

Arguments which form strange reading at the present day were advanced by eminent scientists against the idea that bodies could come to the earth from space, a number of French scholars being particularly vehement in denying such origin. The famous chemist Lavoisier was one of a committee who presented a report in 1772 to the French Academy regarding a stone claimed to have fallen some four years previously at Luce. The report stated that the stone was an ordinary one but that "It had been struck by lightning." It was, however, a true meteorite.

Early historians mention several stones which had been seen falling from the sky, Livy and Plutarch in particular relating these facts. Among the list was one stone which had fallen in

Phrygia and had been kept there for centuries until in 204 B.C. it was removed to Rome with imposing ceremonies. They also tell of a shower of stones near Rome in 652 B.C. and a stone that fell in Thrace in the fifth century B.C. The image of the goddess Diana, the famous shrine at Ephesus, is said to have fallen down from Jupiter, and was probably a true meteorite.

Quite naturally the primitive peoples looked upon the stones falling from the sky as being of miraculous origin and in consequence were led to consider them as objects of worship. Indeed certain tribes and nations, that could not be considered primitive, in India, China and Japan, have viewed these articles with awe and reverence. Rev. George McDougall tells us of the strange fascination the "Victoria" meteorite held for the North West Indians of our own land. They looked upon its removal as most likely to result in fearful consequences to them. It was to them a sort of god, a guardian. It had come to them from the heavens, and as its profile bore the remarkable configuration of the face of a man, we do not wonder at the influence this wielded in the Indian tribes of the West.

It was not until almost the close of the 18th century that we find any intelligent efforts made to ascertain the real facts about these falling stones. In 1794, on the 16th of June, a shower of stones fell at Siena in Italy and the occurrence is thus described by Sir William Hamilton, in connection with his account of an eruption of Vesuvius. (From *Philosophical Transactions* of the Royal Society of London). "I must here mention a very extraordinary circumstance indeed, that happened near Siena in the Tuscan State, about eighteen hours after the commencement of the late eruption at Vesuvius on the 15th of June, though that phenomenon may have no relation to the eruption; and which was communicated to me in the following words by the Earl of Bristol, Bishop of Derry, in a letter dated from Siena, July 12th, 1794. 'In the midst of a most violent thunder-storm about a dozen stones of various weights and dimensions fell at the feet of different people, men, women and children; the stones are of a quality not found in any part of the Sienese territory; they fell

about eighteen hours after the enormous eruption of Vesuvius, which circumstance leaves a choice of difficulties in the solution of this extraordinary phenomenon ; either these stones have been generated in this igneous mass of clouds, which produced such unusual thunder, or, which is equally incredible, they were thrown from Vesuvius at a distance of, at least, 250 miles ; judge then of its parabola. The philosophers here incline to the first solution. I wish much, sir, to know your sentiments. My first objection was to the fact itself ; but of this there are so many eye-witnesses it seems impossible to withstand their evidence, and now I am reduced to a perfect scepticism.'

" His Lordship was pleased to send me a piece of one of the largest stones, which when entire weighed upwards of five pounds. I have seen another that has been sent to Naples entire and weighs about one pound. The outside of every stone that has been found, and has been ascertained to have fallen from the cloud near Siena, is evidently freshly vitrified, and is black, having every sign of having passed through an extreme heat ; when broken the inside is of a light grey color mixed with black spots, and some shining particles, which the learned here have decided to be pyrites, and, therefore, it cannot be lava, or they would have been decomposed."

Scientists are sometimes very hard to convince and some of that day contended that the Siena stone had been formed by the condensation of the particles of dust in an eruption cloud from Vesuvius. Even though the largest stone weighed 7½ pounds and Vesuvius was so distant, the theory was quite unshakable. In the following year, 1795, in Yorkshire, England, a 56-pound stone fell almost at the feet of a laborer. Still this was not sufficient to dislodge from the minds of its originators the theory of condensation. But in 1798 out of a cloudless sky in India came a ball of fire. A series of heavy explosions followed, and a shower of stones fell to the earth. Again in 1803 near Paris, France, after the passage of a swift-moving ball of fire an explosion was heard over an area of 75 miles in diameter. Immediately following this, there fell some two or three thousand

stones within an area of six by two miles. This was indisputable proof. Here again the sky was perfectly clear and competent scientists immediately investigated all the circumstances.

Of the meteorites in existence to-day the oldest of which we have an exact record fell in 1492, on November 16th, in Alsace, Germany. The record states that between 11 and 12 o'clock in the forenoon, with a loud crash of thunder and a prolonged confused noise heard afar off, there fell, at the town of Ensisheim on the Rhine, a stone weighing 260 pounds. A child saw it fall in a wheat field, and when they located it they found it had penetrated the earth to the depth of a man's stature. King Maximilian, who happened to be at the town just then, ordered the stone conveyed to the castle, and after breaking off two pieces, one for himself and one for Duke Sigismund, of Austria, directed that the big stone should be suspended in the parish church. There it remained for three centuries, when for a time during the French Revolution it was carried off to Colmar, but was eventually restored, and to this day Ensisheim rejoices in the possession of this remarkable ærolite.

Whence come the meteors? The answers are numerous and varied. If we accept the theory of a small group of scientists, among them Mr. W. H. Pickering, we will have to believe that the moon was thrown off from the earth ages ago; and in such a gigantic eruption innumerable small pieces were loosened and flung into space. Another school claim the sun as the source, its enormous eruptive power being able to overcome the force of gravity and in consequence these millions of small bodies are flung about the solar system at random apparently. But when it is considered that the astronomer of to-day foretells the date of arrival of "showers" of meteors, in view of his knowledge of the path of a comet, we must accept the relationship existing between comets and meteors. The once majestic comet has become dissipated into innumerable bodies, each a small world, which trail along in the original path. Fully 100 of such paths are catalogued to-day. And we must, therefore, admit

that it is most likely the major number of meteorites were formerly associated in some manner with comets.

There are at present some 700 meteorites, or 700 distinct falls, treasured and jealously guarded in museums and collections. They vary in weight from a fraction of an ounce to that of Ahnighito the wonderful Greenland siderite with its 36½ tons.

Before attempting a description in detail of some of the most famous meteorites let us look for a few moments at the usual classification made according to mineral composition, and to some of the characteristic features.

There are three general classes :—

1. SIDERITE or iron meteorite, which consists essentially of an alloy of iron and nickel (Gr. sideros – iron).

2. SIDEROLITE or iron-stone (Gr. sideros and lithos).

3. ÆROLITE or stone, meaning an "air stone."

The line of demarcation between these classes is not always sharp, and again within these general divisions there are many subordinate classifications.

Meteoric masses are probably extremely cold out in space. But when they enter the earth's atmosphere at high speed the friction with the air combined with the enormous pressure raises the temperature of the surface to the melting point and a state of incandescence, producing a volume of dazzling light. Careful observations lead to the statement that a speed of from 20 to 50 miles per second is common. Thus only a period of two or three seconds occurs between glacial cold and the surface of the earth, if the meteorite should be of sufficient size originally to survive such a journey. And it is very probable that the interior of the meteorite attaining the earth does not even become warm. The Dhurmsala meteorites fell in moist earth, appearing as a dazzling ball of fire. An hour afterwards they were found, and were coated with frost, showing the nature of the former habitat.

The rapid heating of the exterior and the consequent extremes in temperature between various parts of the meteorite, combined with great pressure on the front or face, and a vacuum

behind, generally leads to rupture before reaching the ground. The stony meteorites are particularly prone to such. The rupture is usually accompanied by one or possibly a series of very loud, sharp detonations sometimes heard a hundred or more miles away. The siderites seldom burst, only six such having ever been recorded.

While some forty elements are said to occur in meteorites, the eight most abundant are found in the following order: iron, oxygen, silicon, magnesium, nickel, sulphur, calcium and aluminum. Among the most common elements are many which have a wide distribution and exist in great quantity on our own planet. But in the compounds there is marked dissimilarity. On the earth "free quartz" is the most prevalent compound, entering into the composition of such common rocks as felsite, syenite, gneiss, granite, etc. But this is never found in meteorites. And in meteorites we get compounds unknown to our mineralogy such as Troilite, a combination of sulphur and iron. There is also Schreibersite, a phosphide of iron, nickel and cobalt, a generally disseminated constituent of siderites and forming some of the shining lines to be seen in etched sections.

The iron of meteorites is always very distinctive, as it is not free, but found as an alloy with from 6 per cent. to 20 per cent. nickel. This alloy is usually of a crystalline character, and when cut, polished and etched with acid, a beautiful network of lines is shown varying with the crystalline character of the mass. This network is called "Widmanstätten" figures or lines from the name of their discoverer. When these figures are strongly developed, the meteoric origin cannot be questioned, though their absence does not necessarily disprove such origin.

Troilite, which is a very common constituent of meteorites, is generally considered to be the simple sulphide of iron *FeS*, though the exact chemical composition is in doubt. This is usually in the form of nodules, plates or rods, and decomposing readily during the ærial flight, leaves the remaining mass with unique markings.

Of great scientific value is the fact that hydrocarbons of

various kinds have been found in meteorites. Since they are readily combustible or volatile, it proves that they have not been subjected to great heat. Furthermore the existence of hydrocarbon compounds leads to some speculations as to the associations with life of some kind, as it is usually thought organisms are necessary to the formation of such on the earth's crust.

I have been particularly interested in the collection of meteorites in the American Museum of Natural History, New York. Money has been lavishly spent during the last few years to make it the largest and most complete in the world.

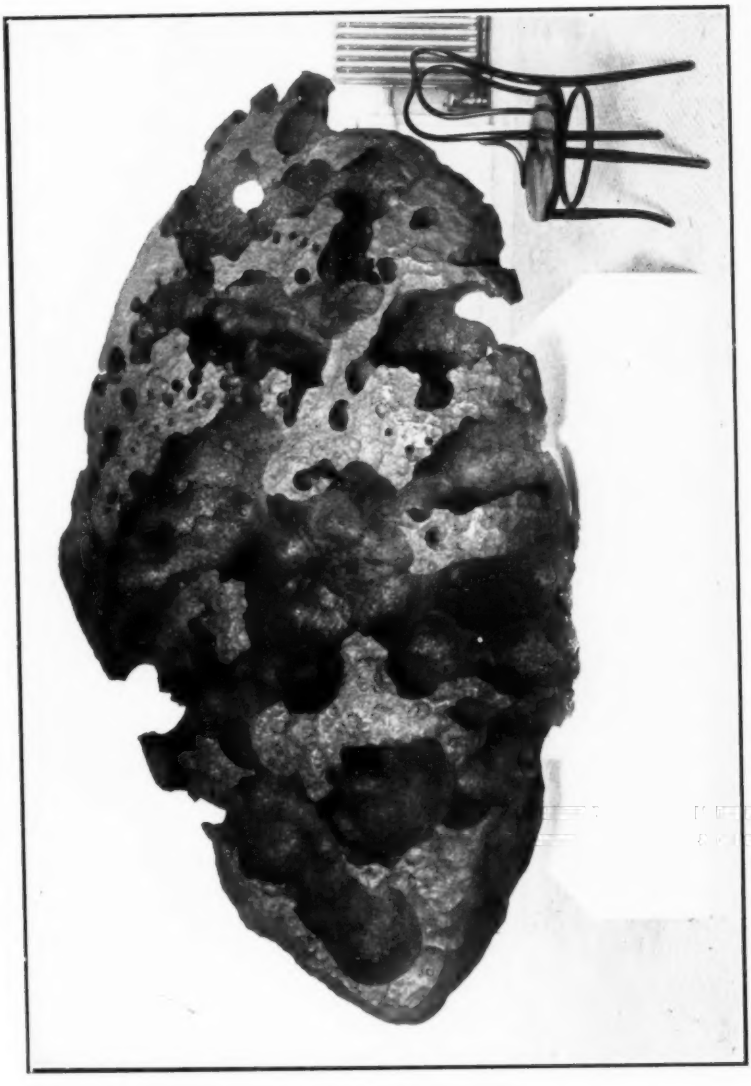
Of special interest is the group of three comprising the "Cape York Meteorites."

Commander Robert E. Peary in 1894, while exploring the Arctic regions, was guided by an Eskimo named Tallakoteah to see the "Saviksue" or great irons, on the north coast of Melville Bay, near Cape York, Greenland, 76 degrees North Latitude. Each had been given a name suggested by its shape, the "Dog," the "Woman" and the "Tent." The following year Peary returned to remove the meteorites. The "Dog," weighing 1100 pounds was soon transported to the ship. But the three-ton mass, the "Woman," arrived there only with great difficulty and much excitement. A large cake of ice was used as a ferry between the shore and the vessel. At the ship's side tackle was being adjusted when suddenly the ice broke in pieces. For the moment all seemed lost. Enough of the tackle however clung until a safe adjustment was made.

Four miles distant on an island lay the "Tent," which was found to weigh no less than thirty six and one half tons! Equipment which he did not have was necessary to handle so great a mass. In 1906 Peary made a special voyage to the North for this prize, but was unsuccessful. A third effort was necessary and in 1907 success was finally achieved, the ship "Hope" bringing to New York City the largest meteorite in the world.

In honor of his daughter who was born within the Arctic Circle the great iron was named "Ahnighito" this being Miss Peary's Eskimo name.

PLATE XII.



THE WILLAMETTE METEORITE

"Ahnighito" measures 10 feet 11 inches by 6 feet 9 inches by 5 feet 2 inches. (Plate XI.).

These three meteorites are very similar in chemical composition, averaging about 91 per cent. iron ; 8 per cent. nickel with small quantities of cobalt, copper, sulphur, phosphorus and carbon. This similarity coupled with their close proximity indicate that they were of the same fall.

How long since these meteorites came to earth is not known. It may be hundreds of years, possibly thousands. It was a popular belief that these masses of iron furnished the Eskimos with material for their knives and other weapons. I failed to find any indications where any portions seemed to have been removed. In fact it would require a machinist's equipment to obtain even a fragment.

As to the value of these three irons I learn from a private source that Mrs. Jesup purchased them from Commander Peary, or rather "Mrs. R. E. Peary," for the sum of $40,000.00, and then presented them to the Museum.

Probably the most remarkable and unique meteorite in existance is the Willamette, also in the collection of the American Museum. It is very large, weighing 31,107 pounds or nearly 16 tons and measures 10 feet by 6½ feet by 4 feet 3 inches. (Plate XII.).

The observer is first impressed by its massiveness, but this soon gives place to wonder and amazement as he notes the deep pits, the series of great hollows, and the "drill holes" so distinctive of Willamette.

One side, with its broad shallow hollows, gives clear evidence of having been the "brustseite," or front, of the meteorite as it sped through the air, the friction with the atmosphere causing the surface metal to melt and flow. The other side is most irregular, having great pot-like pits a foot or more in depth, apparently caused by rusting as the meteorite lay on the ground where it fell. Evidently these cavities were formerly occupied by troilite a sulphide of iron easily decomposed.

One could readily be excused for thinking that a skilled

machinist had been at work with a 2-inch drill as he views a number of cylindrical holes a foot or more in depth. These were the former home of rod-like masses of the troilite.

The history of Willamette is very interesting. It was in the autumn of 1902 when two prospectors looking for gold and silver ore indications, out in the forest about 19 miles south of Portland, Oregon, came upon this mass of iron quite buried in the earth. The meteoric character was soon ascertained. To move the 16-ton mass was a problem, but to get it to the finder's ranch a mile distant without allowing the owners of the land-where they were, to become aware of the fact, presented additional difficulties.

A crude wagon was constructed using cross-sections of a large tree for the wheels, and by much labor and remarkable ingenuity the meteorite was transferred to the desired spot.

The owners of the forest eventually learned the facts and instituted suit in the courts for recovery. A bitter fight was there waged which ended in victory for the plaintiff.

Chemical analysis shows the meteorite to contain about 91·55 per cent. iron and 8 per cent. nickel. A polished and etched surface shows rather broad Widmanstätten lines while the texture is very coarse and granular, the grains being bounded by almost definite planes suggesting crystals.

# THE BOUNDARY SURVEY BETWEEN CANADA AND THE UNITED STATES EAST OF THE ST. LAWRENCE RIVER

By Thomas Fawcett

UNDER terms of a treaty between Her Majesty the Queen of the United Kingdom of Great Britain and Ireland, signed at Washington, August 19th, 1842, by the Right Honorable Alexander Lord Ashburton for Her Britannic Majesty and Daniel Webster, Secretary of State, for the United States.

Article 1 which covers that part of the boundary considered in this paper reads as follows :—

"It is hereby agreed and declared, that the line of boundary shall be as follows :— Beginning at the monument at the source of the River St. Croix, as designated and agreed to by the Comissioners under the Fifth Article of the Treaty 1749 between the Governments of Great Britain and the United States ; thence north, following the exploring line run and marked by the Surveyors of the two Governments in the years 1817 and 1818 under the Fifth Article of the Treaty of Ghent, to its intersection with the River St. John, and to the middle of the channel thereof ; thence up the middle of the main channel of the said River St. John to the mouth of the River St. Francis ; thence up the middle of the channel of the said River St. Francis and of the lakes through which it flows, to the outlet of the Lake Pohenagamook ; thence south westerly in a straight line, to a point on the north west branch of the River St. John which point shall be ten miles distant from the main branch of the St. John in a straight line and in the nearest direction ; but if the said point shall be found to be less than seven miles from the nearest point of the summit or crest of the highlands that divide those rivers which empty themselves into the St. Lawrence from those which

fall into the River St. John, then the said point shall be made to recede down the said north west branch of the River St. John to a point seven miles in a straight line from the said summit or crest ; thence in a straight line in a course about south eight degrees west to the point where the parallel of latitude 46° 25′ north intersects the south west branch of the St. John ; thence southerly by the said branch to the source thereof in the highlands at Metjarmette Portage ; thence down along the said highlands which divide the waters which empty themselves into the St. Lawrence from those which fall into the Atlantic Ocean, to the head of Hall's Stream ; thence down the middle of said stream, till the line intersects the old line of boundary surveyed and marked by Valentine and Collins previous to the year 1774 as the 45th degree of north latitude, and which has been known and understood to be the line of actual division between the State of New York and Vermont on one side and the British Province of Canada on the other ; and from said point of intersection west along the said dividing line as heretofore known and understood, to the Iroquois or St. Lawrence River."

Article No. 6 of the same Treaty made provision for carrying the terms of the treaty into effect by appointing two Commissioners, one by Her Britannic Majesty and one by the President of the United States and the Senate thereof. A section of the Article reads as follows :—

"The said Commissioners shall meet at Bangor, in the State of Maine, on the 1st of May next, or as soon thereafter as may be and shall proceed to mark the line above described from the source of the St. Croix to the River St. John, and shall trace on proper maps the dividing line along said river and along the River St. Francis to the outlet of Lake Pohenagamook ; and from the outlet of the said lake, they shall ascertain, fix, and mark by proper and durable monuments on the land the line described in the first article of this treaty ; and the said Commissioners shall make to each of their respective Governments a joint report or declaration under their hands and seals, designating such line of boundary, and shall accompany such report or

declaration with maps, certified by them to be true maps of the new boundary."

The Commissioners appointed by the two Countries were Lieut.-Col. J. S. Buchnal Estcourt on behalf of Her Britannic Majesty and Mr. Albert Smith for the United States. With a view to carrying out the necessary astronomical work which such a survey demanded, on behalf of Her Majesty's Government, the Earl of Aberdeen, then Secretary of State for the Foreign Department, applied to Professor G. B. Airy, F.R.S., the Astronomer Royal, requesting him to recommend two practical astronomers accustomed to the use of first class instruments since it would be necessary to determine with accuracy the latitudes and longitudes of several points for the purpose of determining the positions denoted in the Treaty. After studying the wording of the Treaty, Professor Airy submitted several questions to the Foreign Office in reference to the wording of certain clauses which he considered required a more definite meaning before he would be prepared to lay out any scheme for the Astronomical Work. The questions by Mr. Airy and replies thereto given by Mr. H. M. Addington, Secretary to the Foreign Office are as follows: The questions refer to the wording of certain sections of the Treaty.

*Question I.*

"Thence north, following the exploratory line run and marked by the surveyors, etc." Is this line to be traced truly north, even though it diverge from that of the former surveyor; or is any respect whatever to be paid to the line of the former surveyors?

*Answer.*

The line of the former surveyors is intended to be adhered to, consequently the term "north" does not mean truly north, but in a northerly direction.

*Question II.*

"Thence southwesterly in a straight line to a point on the north west branch of the St. John, which point shall be ten miles

distant from the main branch of the St. John." Is the line to be traced truly south-west? the point upon the north-west branch of the St. John is a strictly definite point and may not be hit by a southwesterly line,—and does the expression ten miles denote ten miles at the least?

*Answer.*

The term ten miles denotes ten miles at least, according to the English view of the Treaty; consequently the term *southwest* must be taken as meaning not truly southwest but in a southwesterly direction.

*Question III.— last line but two.*

"And from said point of intersection west along the said dividing or heretofore known and understood." The doubt is the same as in question No. 1. If the new line diverge from the old understood line, which is to be adopted?

*Answer.*

The old line is to be adhered to, consequently the term west must be taken as meaning westerly direction."

After receiving those answers Professor Airy compiled a table containing a summary of such observations as he thought would conform with the requirement as set forth in the several answers to the questions submitted.

| SECTION OF BOUNDARY LINE | OBSERVATIONS REQUIRED |
|---|---|
| 1. From Mars Hill near the source of the St. Croix River to the River St. John. | None, the line already traced to be followed. |
| 2. Up the channel of St. John River and of the St. Francis to Lake Pohenagamook. | None. |
| 3. In a straight line to a point on the branch of the St. John ten miles distant from the Main Branch. | None. |
| 4. Thence to the point where the parallel of 46° 25′ latitude intersects the St. John River. | One latitude to be determined. |

| | |
|---|---|
| 5. Thence up the stream along the watershed, and down Hall's Stream to the old line of boundary in 45° N. latitude. | None. |
| 6. Thence along the line marked by Valentine and Collins to the Iroquois or St. Lawrence River. | None. the line already run to be followed. |

This memorandum which was sent to Mr. Addington at the Foreign Office, was forwarded to Col. Mudge, R. E., one of the officers directing the Ordinance Survey of Great Britain, and who some years previous had been in America as a Commissioner connected with the same boundary. Col. Mudge had made out a programme of what he considered would be necessary in the way of astronomical work on different sections of the boundary, which was handed to Professor Airy. In referring to that section of the boundary from the source of the St. Croix River to the St. John, Col. Mudge says "This exploring line although supposed to be straight (a good approximation) was not intended to be final. It is only marked by blazing the trees at certain distances and felling such trees as came in the way." "Although the greater part is now filled up with trees, shrubs and fallen timber which must be cleared away again." "I should think that the line as explored and agreed on in Lord Ashburton's Treaty may be sufficiently made out so as to admit, supposing a good intention to exist on both sides, of the Commissioners fixing certain points at equal distances between the marked trees, which being connected by a straight line will form the boundary line from the monument to the River St. John.

From this due north line to the next point mentiond in the Treaty, it is manifest that the terms of the Treaty cannot be complied with. It is there stated that points or marks in the centre of the bed of the River St. John, such to be the boundary marks, to the second lake on the River St. Francis. Now this is altogether impracticable if the marks are intended to be at all durable. The melting of the ice in the spring would sweep

away anything built on or driven into the bed of the river. The only way to carry out the intention of the Treaty will be to let the banks form the boundary with an equal right to navigation to both parties. This, I conclude (says Col. Mudge) will be finally arranged through Lord Ashburton with the American Authorities.

The boundary line would then be determined from the monument by the "due north" line to the River St. John and from thence up to the south end of the second lake on the River St. Francis, a tributary of the St. John. The straight S. W. line from Lake Pohenagamook to the N.W. branch of the River St. John — a line nearly 70 miles in length through a dense forest which was to be drawn straight so as to connect two fixed points, was a proposition which presented great difficulty and promised to be exceedingly laborious. This was a condition where Col. Mudge recognized accurate astronomical observations to determine the latitude and longitude at each of the points to be connected by a straight line; the difference of latitude and longitude of the two points to be connected would be required to compute the bearing of a line that would join them.

The astronomical observations which Col. Mudge recommended for the purpose of establishing the boundary were the following points at which he recommended latitude and longitude determinations would be advisable.

1st. The monument at the source of the St. Croix commencement of the north line.

2nd. At the termination of the north line touching the St. John River.

3rd. The southern end of Lake Pohenagamook on the St. Francis River.

4th. The point on the N. W. branch of the River St. John to be established ten miles distant from the main branch in a straight line.

5th. The point on the S. W. branch of the St. John inter-

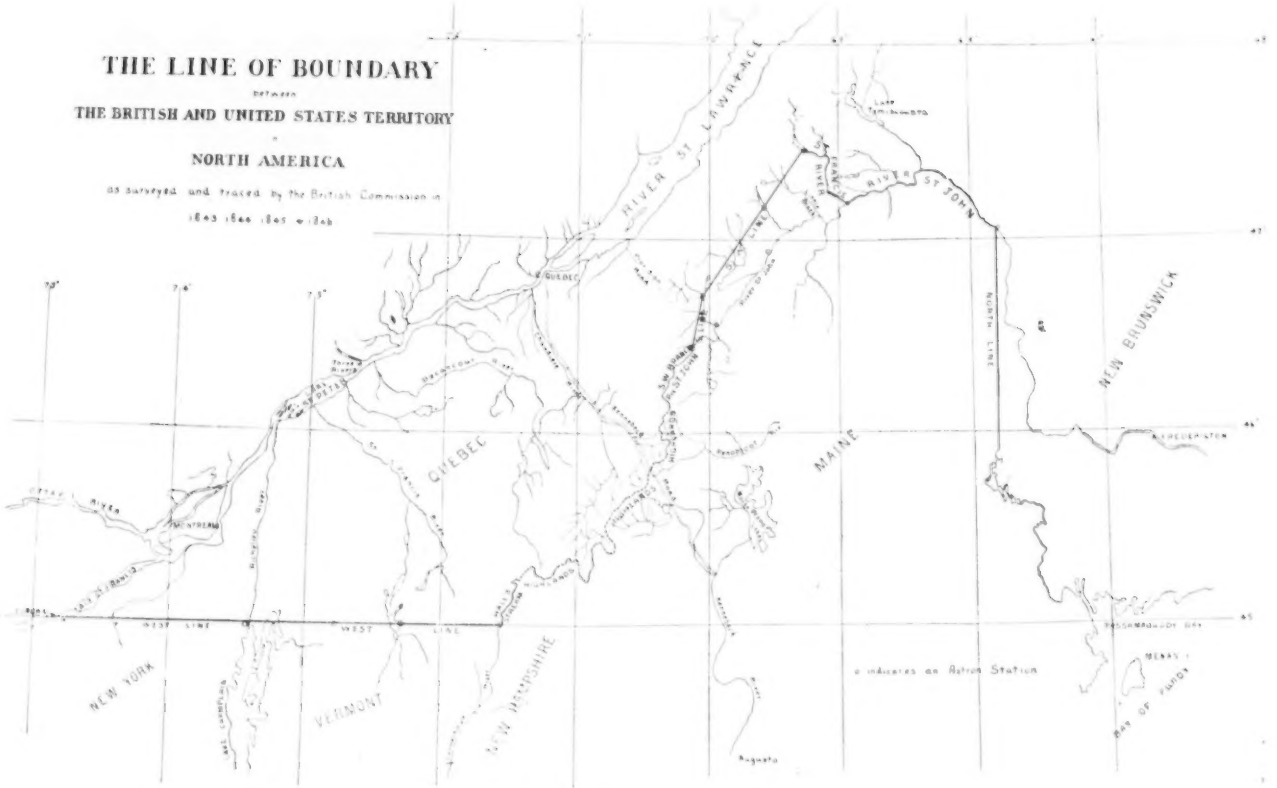
sected by the parallel of 46° 25′ south, eight degrees west from the point before named on the N. W. branch.

6th. The source of the S. W. branch in the highlands and at as many other points as convenience and time of observers would admit.

For the purpose of ascertaining the difference of longitude between Lake Pohenagamook and the north west branch of the St. John River, intermediate astronomical stations were established at the mouth of the St. Francis, on the St. John River, at the rapids about half way to the N. W. branch and at the junction of the Daquamme or Metaquamme with the St. John. The astronomers, Capt. Robinson and Lieut. Pipon, Royal Engineers, occupied the stations at each terminus, and chronometers were sent back and forward in charge of members of the Engineering Corps, by canoe in summer and sleighs drawn by horses after the river froze over, repeating the journeys and comparing the time at the two points, also checking results by moon culminations and lunar distances until the officers were satisfied they had a very close approximation to the difference of longitude between Lake Pohenagamook and the astronomical station at the junction of the Daquamme with the St. John. From that point a line had been cut out and measured to the point selected under the Treaty on the N. W. branch, and with this distance and the bearing of the line the difference of latitude and longitude was computed and the data for determining the bearing of the *south-west* line complete.

The officers then took a trip out to Quebec; this was in the month of January and a road had been opened to Quebec to haul provisions in to the north-west branch. While there they had an opportunity to complete their computations and get everything in readiness to run the south-west line with large parties working from both ends.

Col. Estcourt and Capt. Robinson with a large corps of sappers and miners and laborers returned to the north-west branch the 1st of March and Lieut. Pipon with a smaller party




MAP SHOWING THE INTERNATIONAL BOUNDARY BETWEEN EASTERN CANADA AND THE UNITED STATES

proceeded to Lake Pohenagamook. Both parties began work about the 1st of March. The account given of this work in a letter from Capt. Robinson written to Professor Airy will convey a very clear idea as to the *modus operandi*. The letter says in part—"I have the pleasure of informing you that we have completed the running of the line from the outlet of Lake Pohenagamook to this point. The measured distance is 64½ miles—agreeing closely with the computed one, and the *error* of the course or the distance between the ends of our two lines on the ridge where we met each other was 341 feet. I feel sure we should have been nearer than this but for the circumstances of many of our observations having been made during the winter which was a severe one." "Capt. Estcourt has expressed himself as highly satisfied, but, of course, we cannot venture to take any credit to ourselves until we have your opinion. Trigonometrically done, an error of 341 feet in a side would be very great, but astronomically I hope it will pass creditably." "As soon as we had laid off the direction of the line and had cut some four miles we came to high ground, the line passing over the shoulder of Sugar Loaf Hill. From a point on this we commanded a good view over the country up to 35½ miles on the line, and by raising a platform we saw the 42½ mile point. We pitched our tents on Sugar Loaf Hill, set up the transit and directed the setting of points in the line up to the time of meeting Mr. Pipon from the opposite end." "Our mode of proceeding was as follows:—At the N.W. branch I used the altitude and azimuth instrument as a transit, setting up a meridian mark, and at the same time I used the transit for getting the correct time. The altazimuth instrument stood 35 feet east of the transit, the axis being carefully levelled, and when a circumpolar star crossed the centre wire, it was also intersected by the altazimuth instrument and a meridian mark afterwards fixed under it. This was tried and re-tried with the instrument reversed. The angle was then laid off by the altazimuth instrument and a lamp on Sugar Loaf Hill was lined in by signal and fixed under the centre wire. The transit was then moved up to the point and a good firm stand

made for its reception. Every evening a large lantern with powerful burner and reflector was set up on the station where the altazimuth instrument has been placed as a back sight and which had been decided to be the boundary deflection point on the north west branch. The transit instrument was set and levelled and centred on this light, the telescope was then turned over in the direction of the line towards the next ridge or prominent point — (the cutting party having made their way through the woods during the day to the point) burnt a torch which was easily moved to the exact spot under the centre wire by flashing gun-powder. The signals were thus, one flash — are you ready? which, when answered by a flash, was an affirmative. Then we had one flash, move east, two flashes, move west, three, wait a while, four, all right, go home. Each signal was answered by a corresponding number of flashes from the other party. The flashes were made at first with a quantity equal to two charges of a fowling piece, then three charges, and at the last or greatest distance four charges. With these charges every flash could be distinctly seen, and the party could be moved almost as easily as by speaking to them. Rockets were tried for a while but many of them were never seen, and when seen they were not as good as the gun powder. The torches were made of birch bark saturated with turpentine and rosin and were distinctly visible at the greatest distance, the 42½ mile point. When the sun shone a heliostat was used for lining in. When this was visible through the last opening in the woods crossing a ridge the men could ascertain the position of the line on the ridge ahead and get the point near enough to be ready for the signals. For the first six or seven miles the line was opened out as the work proceeded, intended to be straight but this was too slow and was never sufficiently correct when within sight of the directing transit, so that system was abandoned and the party directed to go ahead from ridge to ridge as best they could with compass bearings, and on the top of each hill where the torch had been lined in, good stout poles with boards nailed across were set up as guiding points between which the sappers' theodolites could direct the

cutting, the whole of the line to be cut out 30 feet wide — eight feet of which was to be cleared to the ground similarly to the north line from St. Croix to the St. John River. Mr. Pipon and I saw nothing of each other's movements until near the place of meeting, my party when making their way up a long hill about three miles from the top saw trees falling and an opening being made on the ridge apparently right in the direction they were going. To intimate to us at Sugar Loaf they had seen something satisfactory, the party on their return had an illumination with torches, and danced a hornpipe which we could see the crossing and dodging about of the lights. In determining the points on continuation of our respective lines it was found that my line was 341 feet to the northwest of the other. In correcting for this error we considered our lines to be running parallel to each other, and divided the distance 341 feet in the proportion of 42½ to 22 — the distance made by each. After two flag poles had been erected on two ridges the cutting parties turned their backs on each other and returned to their respective posts altering their former marks on the several ridges proportionately to the whole error and their distance. This completed, the small transit was sent through the whole way from Lake Pohenagamook to the northwest branch working from point to point and correcting discrepancies so that we were pretty sure of its being straight from end to end."

This part of the line having presented the greater difficulty in its determination, I have taken the account — somewhat abridged—verbatim from Capt. Robinson's letter reporting the survey to Professor Airy, R.A. Professor Airy had nothing but the highest praise for this work which he declared to exceed in accuracy any thing that he had expected, and the astronomers were given great praise for their skill in achieving such satisfactory results from such data as was available to them.

In the appointment of astronomers for this work, the Foreign Office applied directly to Professor Airy, the Astronomer Royal, offering a salary of £600 per annum which would be equivalent to not less than $10,000 or $12,000 if rated at its pur-

chasing value then in comparison with the present time. This, of course, was an inducement for good men to accept the position if offered them.

Professor Airy turned his attention to the Royal Engineers who were conducting the Ordinance Survey of Great Britain for men who had practical experience in the use of instruments of the first-class and who had been inured to camp life, as these would be better adapted for the work than young men connected with the observatories. So he wrote Col. Colby, the Superintendent of the Ordinance Survey and that officer recommended Capt. Robinson and Lieut. Pipon to be well adapted for the work if the regulations under which they served as Royal Engineers could be relaxed so as to permit of their absence from England and employment on the International Boundary Survey. Through the representations of the Earl of Aberdeen, Secretary of State, Viscount Canning, of the Foreign Office and others — the appointments were made; also a number of others belonging to the Corps of Royal Engineers were appointed to assist by taking charge of parties on the work and maintaining discipline. Professor Airy had the two astronomers at the Royal Observatory, Greenwich, acquiring practice and skill in such astronomical work as they had to undertake on the boundary, for some months before the expedition set out. J. D. Graham, Topographical Engineer of the Coast and Geodetic Survey, was the astronomer for the United States and also directed the operations of the American parties in the field. He was a very capable and painstaking observer and was said to be the first field astronomer to determine the latitude by observing the difference of the meridional zenith distances of two stars on opposite sides of the zenith, a system still considered among the most reliable and used almost exclusively in the Coast and Geodetic Survey of the United States. In determining the difference of longitude between points on the 45th parallel, the British and American astronomers worked together, a mountain was chosen which could be seen from both directions by observers stationed from 40 to 60 miles apart, and parties were sent equipped with powder,

rockets and everything necessary to produce flashlights at certain pre-arranged intervals of time which could be observed simultaneously by the observers at the stations for which the difference of longitude was wanted. The results from this method proved very satisfactory.

The renewal of the boundary question arose at the time the Alaska problem was under consideration. The Treaty which arose out of that dispute not only made provision for the delimitation of the Alaska boundary which had not previously been run but for the renewal and permanent demarcation of the lines formerly run from the Atlantic to the Pacific. For the purpose of carrying out this scheme, His Britannic Majesty appointed Dr. W. F. King, C.M.G., Dominion Astronomer, the Commissioner on behalf of the British Empire, and the United States appointed Mr. O. H. Tittmann, Superintendent of the Coast and Geodetic Survey, Commissioner on behalf of the United States. This work has been in progress at different points for several years — carried out by officers under instructions from the Commissioners, the American and Canadian parties often working harmoniously together. The section east of the St. Lawrence forms one of the links in this great chain. The old lines are retraced and opened out to a width of about 30 feet or as wide as necessary for the purpose of having a clear sky line. Along the centre of the line, the brush is cleared away and such logs as would interfere with accurate measurements cut out. The lines are measured with great care where a system of triangulation cannot be carried along as a means of control in determining accurate measurements. The tapes used are *invar* which have been compared with the most reliable standards. The line is prepared for measurement by driving hubs from four to six inches in diameter and rising from three to four feet above the surface of the ground for supports for the ends of the tape. Each hub is braced by three or four braces which are driven into the ground at an angle and nailed to the centre hub near the top which holds it firmly in place. The hubs are set at an exact tape length and a strip of copper or zinc about half an inch wide and

two inches in length nailed on top in the direction of the line for the purpose of marking the ends of the tape. An additional support is set for the centre of the tape when one more than 100 feet in length is used. This may consist of a stake driven in line with the end hubs and a nail driven in at the proper elevation for supporting the tape at the centre. When measuring, the tape is usually handled by four men. Thermometers are attached about one-fourth the length of the tape from each end and read by the men who attend to the marking. Two men carry levers which they connect with the rear end of the tape with a chord and at the front end to a spring balance which is attached to that end of the tape to measure the pull or stress. The tapes are usually adjusted so as to give the proper length at a temperature of 62° F. and a tension of 15 kilogrammes (about 33 lbs.). While the marker at the rear end of the tape regulates the distance until the mark at the end coincides exactly with the mark on the copper strip the marker at the fore-end with a sharp brad marks that end as soon as the balance shows the proper tension and the signal "all right" is given by the other marker. Both thermometers are then read and noted and the tape carried forward to the next hub where the process is repeated until half a mile or sometimes a mile has been measured in one direction, then the same process is followed measuring back — the measurements being made in both directions.

In returning it sometimes becomes necessary to set back or forward an inch or so on the hubs that the marks at the end of the tape may always be on the zinc or copper strip. A scale of parts is carried for measuring these small distances which are entered in the notes as set back or forward as the case may be.

For the purpose of deducing the horizontal distance the difference in the elevation of each hub is taken with a spirit level then the corrections are for slope, for temperature and for distance set back or forward, also for the tape constant. Levels are also taken on cross section lines run out for half a mile on each side of the main line, as often as may be found necessary to secure data to place 20 foot contours accurately on the maps.

The line is permanently marked by carefully preserving the positions of the old monuments by means of a frame work which is erected over the spot and a nail so placed that when a plumb line is attached the plummet will hang exactly over the centre of the monument. A hole is then excavated to a depth of four feet and three feet square unless the solid rock is met with to prevent excavating to that depth. A base consisting of rock, gravel, sand and cement in proper proportions is prepared and the iron post, after being filled with cement, is replaced and carefully plumbed so that the centre is replaced as it stood originally. When the weather is favorable for cement work not more than six sacks (about 550 lbs.) are required for each monument but in the fall when it begins to freeze and the cement sets slowly two or three sacks additional are used. When owing to high hills or ridges the monuments found on the ground are not intervisible, monuments consisting of cement about twelve inches square and terminating in a point are used. These monuments are first cast in a frame and are reinforced with four iron rods one near each corner. These monuments are so set that they rise about ten inches above the base, or altogether about two feet above the ground. The base of all monuments is in the form of a pyramid rising about six inches from the four sides to the centre. These monuments are so placed that they are intervisible and at some points several monuments can be seen with a telescope in both directions.

Where the boundary is defined by the centre of a stream a system of triangulation has been carried along using the quadrilateral system so that distances are determined by two independent means and also the latitudes and departures; the level lines are run for the purpose of determining the elevations above sea level and marking the contours on the maps. For the purpose of carrying this triangulation along streams through the woods, lines had to be opened out for both the sides and diagonals of the figures so as to measure all the angles; and additional bases measured with great care at frequent intervals. At the intersections of lines, hubs, similar to those described above for

chaining, were set and braced so as to be immovable, and a small flag set on top for sighting when measuring angles. When the distances did not exceed 1000 feet in length a small white peg set in the top of the hub or a four inch nail painted white would usually be sufficient. Reference monuments numbered and constructed the same as those cement monuments on the straight lines were set on both sides of the river at distances such that they will always be intervisible when the woods are cleared away and the points require to be used in defining positions on the boundary. The geographical positions, *viz.*, latitudes and longitudes, also interbearings of all these measurements, are computed, and by means of a table to be compiled, points on the boundary as laid down on the charts compiled by the former commissioners can be determined.

Along the north line from St. Croix and along the St. John River where the country is more or less cleared, the plane table was used in determining the topography and very good charts showing the locations of all the houses, fences, roads, railroads, etc., in addition to the contour lines were made.

The same condition, but to a lesser degree, held along the St. Francis River. The character of the country from Lake Pohenagamook to Hall's stream is very much the same as it was seventy years ago. In the United States the northerly part of the State of Maine belongs to private companies who preserve it as an unbroken forest for the production of lumber and timber and they cut over different parts by rotation taking only such timber as is mature. Adjoining the line in Quebec, settlement is excluded the greater part of the distance. At a few points as at Lake Pohenagamook, St. Pamphile, the Kennebec Road and probably two or three others east of Hall's stream a road has been made to the boundary and settled, but by far the greater portion is inaccessible from any settlement and the timber under limit which does not call for any speedy removal — so the wilderness remains. That part of Canada is subject to quite a heavy precipitation. During the winter the snow usually falls to a depth of from three to four feet, covering the ground from Nov-

ember until the month of May, and the summer months are usually noted for abundance of rain, so the forest fires do not make such headway in denuding the forests here that they do in many other parts of Canada where moisture is absent and the precipitation less. The owners of the land in the United States keep bush rangers or fire guardians who travel the country over and are on the look out for any fires which may be accidentally started by lightning or otherwise and to extinguish them or secure such assistance as may be available to keep the fire from spreading, so that in many parts of the country there are no evidences of it being overrun by fire since the line was run out seventy years ago.

OTTAWA, CANADA,
April, 1913.

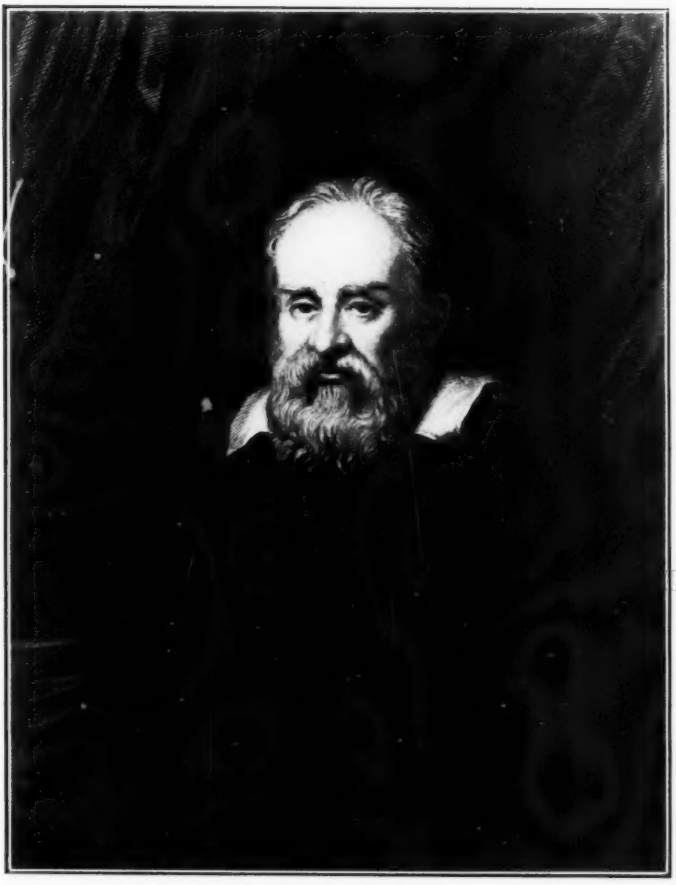
# GALILEO

## A MONOLOGUE

By Albert D. Watson

It is a strange misapprehension — and
Small wonder in a world of sense — that all
That moves *with* us seems motionless, while all
Apart from us appears processional.
The common mind thinks all beneath is fixed,
Deems all above a flux, so the unseen
Seems negligible, evanescent, aye,
Unpractical.
Quest of reality
Means progress, but discoverers disturb
Opinions and, to all appearances,
Unprop the world. When truth invincible
Bears down into an ocean of wrong thought,
And all the fixed conviction of the past
Is staggered, jolted, dislocated, found
Dissolved, it is small wonder if the priests
Should kill the prophets, thinking thus to stem
The rivers of inrushing light.
Set free
The wonder in the soul, that all may know
How this green, hanging-garden of a world
Is swinging on through space, upheld by law
Or — what you will — a hand, a heart, a God
Who holds His star-dominions up and bears
Them on through the ethereal deep. But if
Upon a cross of prejudice you nail
That wonder, when a thousand years have passed,
Men will, as now, be ignorant and blind,

PLATE XIII.



GALILEO (1564–1642)

From a steel engraving of a picture by Ramsay in Trinity College, Cambridge.

*Journal of the Royal Astronomical Society of Canada, 1913*

Assuming still that all that is unseen
Is nought.
Here stop and think. What if the laws
That move the pendulum, the arrow and
The star are but one law which neither I
Have found, nor Kepler, nor Copernicus?
They both are dead, and I am old. These eyes,
That saw so much ne'er seen before, are blind.
These ears are deaf, that suffered not the lips
To speak until I heard God's voice. This form
Is vexed with twinge and disability,
But soon a greater Kepler may arise,
A new Copernicus to bind, with one
Great law, the atom to the star and find
One order of majestic harmony
Throughout the universe.
No falsity
Can choke God's truth; no prejudice can dim
The white light of His law. A fact when found
And firm-established by experience
Can have no sudden revocation. Who
Shall countermand a fast-entrenchèd law?
No prestige of authority can save
A lie — though forced from Galileo's lips
By fire — amid the drift of winnowing light
That sifts the truth as on the wind-swept hills.
The formulas of men, by councils sealed,
Must by experience be signatured,
For only thus the authenticity
Of God's own word is known.
Now I must rest,
So sore I feel the burden of the years.
My force is spent. Be good enough, I pray,
To lead me to my couch.

# THE ASTRONOMICAL WORKS OF OLAUS RŒMER

By G. Van Biesbroeck*

OLAUS RŒMER, the Danish astronomer, born in 1644, at Aarhus, died in 1710, at Copenhagen, is one of the most interesting figures which marked the beginnings of modern practical astronomy. His name is especially well known since we owe to him the first determination of the velocity of the transmission of light in space, an important physical constant, which he succeeded in establishing by means of the eclipses of Jupiter's first satellite. By this discovery he became illustrious among physicists as well as among astronomers.

Rœmer began his studies at Copenhagen. In 1671 the French astronomer Picard went to Denmark with the double purpose of procuring an exact copy of the observations of Tycho Brahe, who died in 1601, and of determining the geographical position of the celebrated observatory of Uraniburg which the latter had constructed on the Isle of Hveen. Rœmer, who, under the direction of Erasmus Bartholinus, was then occupied in putting in order the manuscripts of Tycho, was detailed to assist Picard in his geodetic operations. The young Dane so distinguished himself in this work that when Picard returned to France he insisted on Rœmer accompanying him. By this circumstance the latter was put in contact with the men of mark whom Louis XIV., on the initiative of Colbert, had recently (1666) grouped into an Academy of Sciences. Already the Academy had elected some foreigners, the most noted of whom were J. D. Cassini, who became the first director of the Paris Observatory, Huygens and finally Rœmer, who soon distinguished himself by his ingenious inventions. He worked at the

*Translated from *Ciel et Terre*, the Bulletin of the Belgian Astronomical Society for May, 1913.

operation of levelling which required the "establishment" of the waters at Versailles, and he was also appointed instructor of the dauphin.

It was in 1675, when he had been three years in Paris, that he communicated to the Academy his discovery of the velocity of transmission of light. The satellites of Jupiter, discovered by Galileo and others about 1610, had attracted the attention of most astronomers, and Cassini had calculated tables for them which were regarded as the best at the epoch which now occupies us. At the Paris Observatory these satellites were followed assiduously. It was in comparing the results of observations with the predictions according to Cassini's tables that Rœmer came upon the traces of his discovery: he showed that before an opposition of Jupiter the disappearances of the first satellite arrived too early while after this moment the reappearances of the same satellite became more and more late. He explained these systematic anomalies by the successive transmission of light, of which he determined the numerical value. It was an idea rather advanced for the time and he was not surprised that it was not accepted immediately, especially as it appeared to be in contradiction with the views of Descartes.

In 1681 Rœmer returned to his own country, where he was received with great honor and was appointed director of the Copenhagen Observatory. This institution was one of the oldest in Europe and presented an appearance sufficiently peculiar; it was in the form of a monumental tower, more than 35 metres in height and about 15 metres in diameter. Instruments similar to those which Tycho had employed a century before were installed upon the upper platform, which was reached not by a ladder but by a helicoidal inclined plane. This means of access was so spacious that in 1716 Peter the Great ascended the tower on horseback, the Empress Catharine followed him in a four-wheeled carriage drawn by six horses; the entire equipage went to the top of the Observatory, paraded about the platform and re-descended without getting tangled up. Rœmer decided that this building was not suited to the work which he had projected.

The ascension was not without difficulty, the instruments and the observers were too exposed to the inclemencies of the weather, and, further, Rœmer was little satisfied with the old instruments which he found there. These were often masterpieces from the artistic point of view, but ill adapted to what he proposed to do. We see him then constructing with his own hands an instrument of a new type, which he installed provisionally in his house. It was the first *meridian telescope*, and by its aid Rœmer determined absolute positions and made great efforts to detect stellar parallaxes. Later, in 1704, he devised the *meridian circle* at his country observatory established in the suburbs of Copenhagen. It was an entire revolution in the art of observing. Rœmer undertook important work with this instrument; from the slight fragment which alone has reached us we know that these observations were the most precise of the epoch and that their loss retarded by a half-century our knowledge of the proper motion of the stars.

Rœmer published little upon his researches. Almost all that we know has been transmitted by Peter Horrebow, his faithful disciple. Horrebow was not only the successor of Rœmer as director of the Copenhagen Observatory, but he became, so to speak, his scientific heir. He gives in his *Basis Astronomiæ*, published in 1735, an interesting description of the instruments and methods of the master. Horrebow was not a man of the calibre of Rœmer; his works — he published twenty-three and had twenty-three children! — are often obscure and prolix; often he understood imperfectly the methods of the master and from observations of the latter he drew conclusions which Rœmer certainly would not have been rash enough to publish. But these works are very valuable because they include most of the details upon the works and the personality of the master.

One of the chapters of the *Basis Astronomiæ* is curious reading: it relates to the annual parallax of Sirius and Vega. Horrebow explains there at length the means which Rœmer wished to employ to determine them. All at once he interrupts his explan-

ation by this note, written under the impression of the moment: "But what noise do I hear? A tumult of people running in the street, of cries, of rolling of the drum and of clanging of the bell! A great fire has broken out. October 20, 1728, 7 p.m." Then follows an account of this fire, which destroyed a large portion of the city of Copenhagen. Horrebow's house was reduced to ashes after his family had escaped with difficulty; his manuscripts, snatched with great difficulty from the flames, were placed in haste in the Observatory, where, a short time afterwards they were nevertheless reached by the fire. Horrebow recounts with many details, how, with tears in his eyes he could only stand helpless at the destruction of the results of long years of labor of his master and himself.

I give here this description in order to explain how it is that nearly all the original writings of Rœmer disappeared long since and that we thus know them by citations at second hand. One manuscript, however, was not destroyed: a collection of Rœmer's notes, gathered by him into a portfolio bearing the inscription *Adversaria* was transmitted in 1739 by Rœmer's widow to the library of the University of Copenhagen. It is not known how this document, which still exists, escaped the fire. In 1910 the Danish Society of Sciences decided to publish the manuscript* on the occasion of the bi-centenary of the death of the author and opened up an elucidation of the astronomical contents of the *Adversaria*.

Drawn by the original personality of the author I devoted myself to this analysis† ; I was surprised to find in the manuscript of the Danish astronomer several interesting and new points of history.

The inconvenient form of the manuscript explains in great

* *Ole Romers Adversaria*, med Understottelse af Carlsbergfondet udgivne af det Kgl. Danske Videnskabernes Selskat ved Thyra. Eibe og Kirstine Meyer, Kjobenhavn, 1910.

† This work, crowned by the Danish Society of Sciences, was made with the collaboration of M. A. Tiberghien, assistant curator of the Royal Library; it appeared also in the *Oversigt* of this Society.

part why it remained almost ignored: it is, in effect, a collection of notes, a species of journal where the author set down his thoughts; these latter are presented without order, refer to questions the most diverse and evidently were not intended for publication. They relate especially to astronomical subjects, but we also find here the views of the author regarding questions of physics, mathematics, etc. I shall confine myself to the consideration here of a few of the paragraphs which have appeared to me particularly interesting.

1. THE DETERMINATION OF GEOGRAPHICAL LATITUDE

One of the simplest, and at the same time the most precise, means of determining the latitude of a place is that known as the *Horrebow-Talcott Method*.

Suppose that we have found that two stars culminate, one to the north, the other to the south of the zenith, at the same height above the horizon. If $\delta_1$ and $\delta_2$ are the declinations of these stars, and if $\phi$ is the latitude of the place, we have

$$\delta_1 - \phi = \phi - \delta_2,$$

and consequently,

$$\phi = \tfrac{1}{2}(\delta_1 + \delta_2).$$

Everything consists then in finding a couple of known stars presenting the peculiarity that they culminate at equal angular distances from the zenith. In general this condition will be fulfilled only in an approximate manner. In consequence of the motion of precession and of other apparent displacements of the stars upon the celestial sphere, the condition satisfied rigorously at one instant will be no longer so at the end of a certain time. Hence the method arose of measuring, with a suitable micrometer, the difference of the meridian altitudes of the two stars considered and then correcting the result. As this operation can be done with sufficiently great precision, even with a small instrument, the process thus modified leads again to the same end. It has the advantage of giving a result not depending on the reading of graduated circles; in effect, the elevations themselves do not

enter but only their difference. The influence of refraction, an element always a little uncertain, is also eliminated. Similarly with the flexure of the instrument. It suffices to have a telescope which can turn about a vertical axis, so as to assume, now to the north, now to the south, the same elevation above the horizon. One assures this by means of a delicate level, securely clamped to the telescope, after the latter has been pointed to the desired elevation.

This process, again, is employed in numerous astronomical and geodetic operations. By its means has been demonstrated, in an unquestionable manner, the displacement of the axis of rotation of the earth, producing the phenomenon of the variation of latitude. It is by this same method that, following an international agreement, a certain number of stations, established especially for this purpose, have studied for several years those delicate variations of the terrestrial pole, the cause of which is still enshrouded in much mystery.

Who was the first inventor of this proceeding? Talcott, an American officer, perfected the method, at the middle of the nineteenth century, by the addition of a level to the elevation telescope; but we must assign to Horrebow, the disciple of Rœmer, the honor of having first applied what we now call the *Horrebow-Talcott Method*. Horrebow first published his proceeding in 1732*:

"I communicate here," said he, "and recommend for general use a new method of determining the latitude by reciprocal elevations."

Our astonishment was great, in perusing Rœmer's manuscript, to find explained there the principle of the same method (on pages 6 and 7). He proposes to apply it in making use of a star culminating to the south at the same elevation as Polaris to the north, or of awaiting the favorable moments when the sun

* In his *Atrium Astronomiæ*, p. 50. One sees here that, already in 1729, Horrebow had communicated his method to DeLisle. This work was reprinted in Volume III. of the *Opera Mathematico-physica* published at Copenhagen in 1740-42.

culminates at the same elevation as Polaris. He says explicity that the method has the advantage of not requiring circles graduated with precision, and that it eliminates the influence of refraction. He speaks of having applied this method in the country and briefly describes an instrument which he imagined for the purpose ; it is precisely the " telescope of corresponding elevations " which Horrebow describes in the *Basis Astronomiæ* (Fig. 1*a*), and which he appears to have employed especially for the determination of time. The observing telescope *A C* is fixed upon a circle, rigidly attached to a rod maintained vertical by a weight, and can turn about the pivot *I*, where by a bracket it is attached to a wall. On making the telescope with the rod to

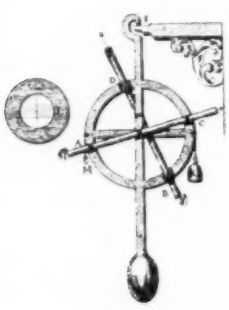


Fig. 1*a*.

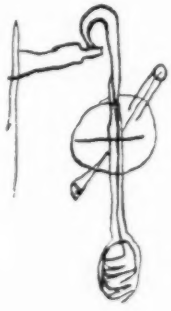


Fig. 1*b*.

RŒMER'S TELESCOPE OF CORRESPONDING ELEVATIONS

turn, the vision is directed always at the same elevation, now to the north, now to the south. At the focus of the telescope is placed a reticule *L* formed of a series of horizontal parallel wires, the known intervals of which serve to estimate differences of elevation. Horrebow, following Rœmer's example, employed this telescope to determine the time by the method of corresponding elevations, in which he used the movable weight *K*, but we shall not here dwell upon this detail. Beside the design published by Horrebow we have reproduced (Fig. 1*b*) a slight sketch from the *Adversaria*. The first figure has been reduced to the

OLAUS ROEMER (1644–1710).

The famous Danish Astronomer

(By courtesy of *Popular Astronomy*).

same scale as the second. We see that there can be no doubt as to the identity of the two instruments.

It is strange that Horrebow, who worked long years with the master, does not appear to understand that Rœmer had thought, long before him, of the method which he announces as his own discovery. It would be unjust, of course, to suspect Horrebow of having appropriated an original idea which Rœmer had nowhere published. Everywhere the disciple speaks of the master with much veneration; he is only his mouthpiece; he insists, moreover, that he has no other merit than to explain systematically what the master, cut off by death, had not had time to publish. In this case, however, he speaks as if he himself had first discovered the method which occupies our attention.

As we come to see, it is indubitable that the first idea is due to Rœmer. Historically, it is then more exact to speak of the Rœmer-Talcott, rather than the Horrebow-Talcott, method for the determination of latitude.

### 2. THE TRANSIT OF MERCURY OF MAY 5, 1707

Let us see now that portion which relates to the transit of Mercury of May 5, 1707. Since, in 1631, Gassendi was the first to observe a passage of the planet Mercury across the disc of the sun, these phenomena, sufficiently rare, had been followed with the most lively interest. Their observation constitutes an excellent means of correcting the elements of the motion of Mercury, a planet difficult to observe at ordinary times. On this account the tables of the motion of this body were still far from being precise, and one met considerable differences when one sought to predict the circumstances of the transits of Mercury in using one ephemeris rather than another.

Rœmer's manuscript gives indications of laborious calculations which he had undertaken in order to predict as well as possible the conditions of visibility of the phenomenon in 1707. He arrived at the conclusion that the middle of the transit would take place during the night of May 5-6 of this year, and that it would be invisible at Copenhagen. As there would elapse several

hours between the ingres and egress of the planet, the only chance of seeing a part of the transit would be in the neighborhood of sunset on May 5 or of sunrise on May 6.

In effect, on the morning of May 6, Roemer, watching the sun which rose in a clear sky, perceived at one moment the planet just at the edge of the sun. The latter had risen only a few minutes when Mercury reached the edge of the disc and at once disappeared. Roemer, alone amongst all his contemporaries, had been successful in seeing the rare phenomenon. In France, Italy and Germany, notwithstanding a favorable sky, the observations were all negative. Neither on the evening of May 5 nor the morning of the 6th were the astronomers of these countries able to observe the planet.

It appeared to me interesting to investigate, with the assistance of modern tables, how the phenomenon was presented in different countries. I have employed, for this purpose, the tables of the sun and of Mercury prepared by Newcomb in his *Astronomical Papers*. They give the following elements for the centre of the earth :—

| | | | | | | | |
|---|---|---|---|---|---|---|---|
| First contact, | 7h 33m·5, | Greenwich Mean Time, | May 5, 1707 | | | | |
| Second " | 7h 30m·5 | " | " | " | " | " | |
| Third " | 15h 26m·4 | " | " | " | " | " | |
| Fourth " | 15h 29m·4 | " | " | " | " | " | |

At the moment of first contact, May 5, 7.33 p.m., the sun set upon a line passing through Stavanger (Norway), the middle of England, Brest and the west point of Spain. In all the countries situated to the east of this line,—Russia, Germany, Denmark, Italy, the greater part of France and Spain,—the sun had already set when the planet entered on the solar disc. On the contrary, in the western half of England, Scotland and Ireland, the entrance of the planet could be seen a few minutes before the setting of the sun.

Similarly, at the moment of the last contact, May 6, 3.29 a.m., the sun was on the horizon for all points on a line traversing obliquely Europe from Bergen (Norway) to Athens, passing in the neighborhood of Berlin and Vienna. To the east of this

line,—in Sweden, Denmark, in the eastern part of Germany and Austria, as well as in the lands of the East,—the end of the transit could be seen during a certain time after the rising of the sun; while the egress was invisible in England, France, Italy and the greater part of Germany.

These results of calculation accord with the results obtained in these different countries; they explain the lack of success of the observations in France, Italy, and Germany; and show how, thanks to a favorable geographical position, Rœmer was able to see what no one else in Europe was able to discern.

I have calculated, in addition, the circumstances of the phenomenon especially for Rœmer's observatory. The third contact was made May 6, $4^h\ 19^m{\cdot}8$ a.m. (true local time), whilst the end of the phenomenon, or the fourth contact, took place at $4^h\ 22^m{\cdot}8$ a.m.

These data correspond remarkably well with the results obtained by Rœmer. These latter are inscribed on the curious diagram (Fig. 2) reproduced herewith; it represents Mercury leaving the solar disc. An oblique line bearing the numbers 4, 3, 2. . . . . adds the successive positions which, according to the author, the planet occupied from hour to hour during the night of May 5-6. This straight line ends at a more prominent point bearing the entry "$4^h\ 18^m$ observed" corresponding to the spot where the planet appeared to the eyes of Rœmer, showing that it was already almost at the limb of the sun. Across the figure the author notes: "Observation of Mercury leaving the sun the 6th May, $5^h\ 20^m$ a.m.;" and higher up, "Egress was estimated beforehand at $4^h\ 19^m$." We see that he hesitated to fix the moment of egress to a minute; if, on the other hand, we remark that, according to calculation, the sun had appeared at the horizon only a dozen minutes before, there is no need of astonishment at his hesitation; the author, however, adds as a remark, "inter undulas crispati solis," to show how uncertain and difficult was the observation. It would be quite natural, on account of these circumstances, if the planet itself escaped some instants before its true disappearance.

An examination of the annexed figure leaves no doubt as to the authenticity of Rœmer's observation. This sketch is the more interesting as, not knowing of its existence, the celebrated English astronomer Halley placed in doubt the result of Rœmer,

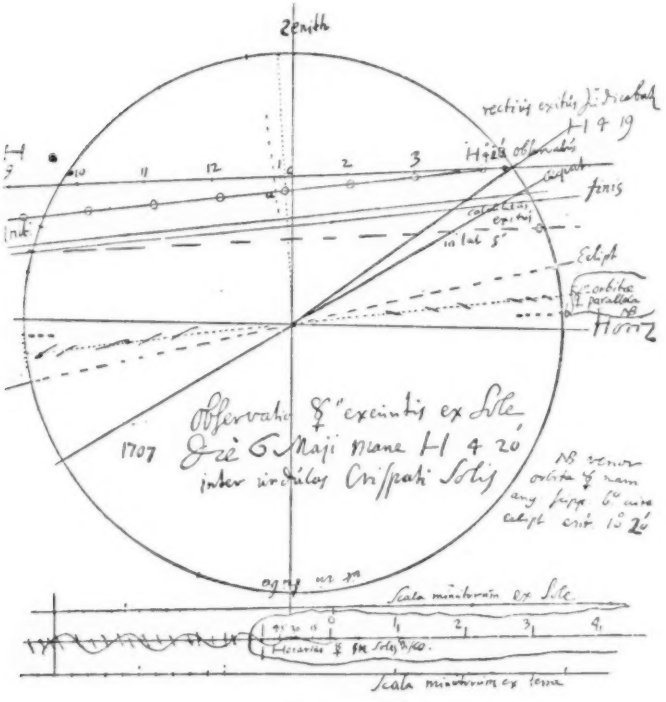



FIGURE 2.

RŒMER'S DRAWING REPRESENTING THE TRANSIT OF MERCURY OVER THE SUN'S DISC IN 1707

who, in certain points did not agree well with his own predictions. Still more interesting is it since in 1835 Baily,* studying

* BAILY, "Some Account of the Transit of Mercury over the Sun, observed by Mr. A. Sharp, in the year 1707." *Monthly Notices of the R.A.S.*, Vol. III., pp. 105-107. London, 1835.

the correspondence of Flamsteed, found there an indication of a problematical observation of the phenomenon in England, differing notably from that of Rœmer. The English observation was due to Abraham Sharp, a student and fellow-worker of Flamsteed, who resided then at Little Horton, near Bradford. Sharp wrote, in effect, to Flamsteed that he perceived the sun at $4^h\ 52^m{\cdot}5$ a.m., May 6, setting out from a bank of clouds at the moment when the planet was on the point of leaving the solar disc. But he presented his observation with much reserve and was not sure that the black spot which he saw was truly the planet. Baily concluded somewhat too quickly that Sharp's observation was a real one since it appeared to him that the position of the pretended planet, indicated by the latter, corresponded better with Halley's prediction than with Rœmer's indication. But he passed over in silence the fact that the time given by Sharp differed by an hour and a half from that noted by Rœmer, when one takes into consideration the difference in longitude between the two observers. An error by Sharp in noting the time could not explain these things, for we have seen above that the egress of the planet was entirely invisible in England. The planet had already left the solar disc when this latter appeared well above the horizon of Little Horton where Sharp observed. In no way could the latter have seen Mercury upon the sun, and it is wrong that one should wish to oppose his witness to that of Rœmer, who is in perfect agreement with modern data.

The only reproach which one can apply to the Danish astronomer in his too great modesty, for he published no part of his observation which he considered at all uncertain. He preferred, he says, to await confirmation from other quarters. This confirmation evidently not arriving, since the phenomenon was invisible in central and western Europe, where alone observers watched for it, Rœmer allowed his observation to be forgotten. It was only after his death, when the French astronomer Delisle, passing through Copenhagen about 1720, found this observation in Rœmer's records, which were then intact, and communicated it to Halley, among others. Without the publication of the

*Adversaria* the doubt on the subject of the observation would still remain.

### 3. RŒMER'S MERIDIAN CIRCLE

Finally, we shall say some words regarding the manner in which Rœmer took account of the instrumental corrections of his meridian circle, the first instrument of this type, which he had invented and installed in his country observatory at Pilenborg, in the suburbs of Copenhagen. Horrebow gives a description of this instrument in his *Basis Astronomiæ*, from which we extract the figure 3, herewith. There is seen in *A D* the telescope tube which turns in the meridian plane by rotation about

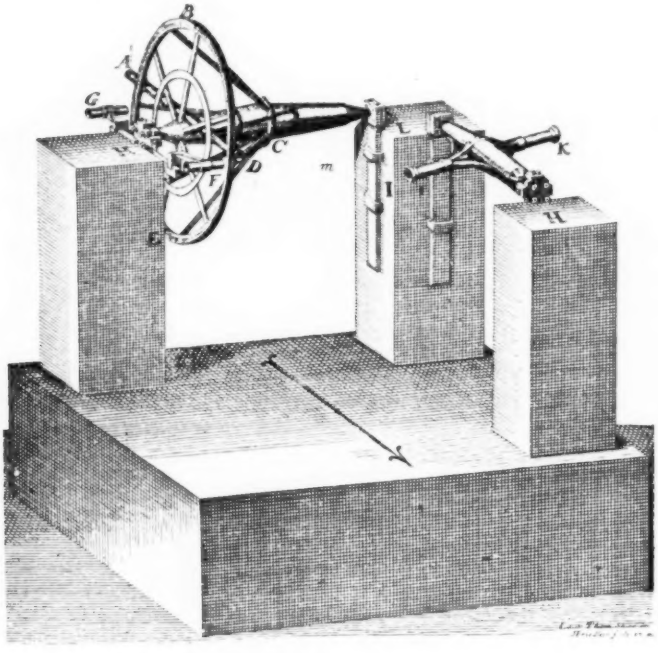



FIGURE 3

RŒMER'S MERIDIAN CIRCLE

the horizontal axis *C*. Achromatic lenses were not known at this epoch ; the only means of reducing the chromatism consisted then in giving a very weak ratio of aperture to the objectives. This is why the tube of the telescope appears so unreasonably long in proportion to its diameter. The telescope is fixed upon a circular limb *A B D E* which has the form of a wheel with six spokes ; this limb bears the graduation permitting one to read the angular elevation of the instrument above the horizon ; six oblique braces join the circle to the middle of the axis of rotation, thus rendering it very solid. Two fixed microscopes *G* and *F* carried by the pillar *H* serve to read the position of the circle.

We see immediately that this instrument, which the inventor named *Rota meridiana*, would give at once the times of transit and the elevation of the stars in the meridian ; it allows then to fix directly their absolute position in right ascension and declination. At an epoch when the positions of the stars were still determined by vertical quadrants, zenith sectors or other instruments employed out of the meridian, the innovation of the meridian circle was of capital importance in the astronomy of position, and one can say that it was in his peaceful retreat at Pilenborg that Rœmer laid the foundations of modern practical astronomy.

The figure herewith shows on the right another simpler telescope serving to observe transits in the prime vertical ; we shall not here dwell upon the advantages of this arrangement for researches such as the fixing of the moment of the equinoxes, the determination of the solar parallax independent of refraction, and other subjects in which Rœmer showed his remarkable powers of intuition.

Let us return again to his manuscript and consider a little the pages 231 to 234, entitled *Machinæ Pilenburgicæ Errores*. The text is accompanied by geometrical figures of which we reproduce the three most interesting ones. (Figures 4, 5, 6).

Just as we do now-a-days, the author distinguishes three

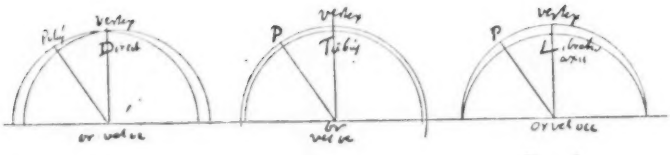



FIG. 4 FIG. 5 FIG. 6

ILLUSTRATING RŒMER'S DETERMINATION OF THE ERRORS OF HIS MERIDIAN INSTRUMENT

instrumental errors which cause the instrument in different fashions to vary from its theoretical position.

There is first the departure of the axis of rotation of the instrument from the east-west direction; this, denoted by *D* (*directio*), is nothing else than what we call the *azimuth* of the instrument. The author here illustrates this in fiigure 4, representing a projection of the celestial hemisphere upon the meridian plane. The diagram shows that, on account of the error of azimuth the instrument describes a great circle passing through the zenith (*vertex*) but making a certain angle with the meridian; this latter is indicated by the semi-circle passing through the celestial pole (*polus*).

Then there is the lack of perpendicularity of the telescope tube — or rather of its optical axis — to the axis of rotation of the instrument. This we denote by *collimation* and Rœmer named it *T* (*tubus*). Figure 5 shows at once that the instrument describes, on account of this effect, no longer a great circle but a small circle parallel to the meridian and near to it.

Finally, the lack of horizontality in the axis of rotation causes the instrument to describe a great circle cutting the meridian at the north and south points of the horizon, as figure 6 shows. This third instrumental error, which we know under the name of *level*, is called by the author, *libratio axis* (*L*).

Rœmer tried to reduce these three instrumental errors to small quantities, azimuth by observing the upper and lower transits of circumpolar stars, collimation by reversing the instrument pointed at a distant object, and level by means of a plumb-

line. He knew that it was difficult, if not impossible, to reduce these errors to zero and to maintain them so: hence, to avoid constantly changing the adjustments, he determined the value of the instrumental errors and applied by calculation the corrections to be made to the observations in order to take them into account. This was a notable advance at this epoch.

He assumed that the three instrumental errors were each one minute of an arc and put in the form of a table the corresponding corrections to the various declinations of the stars observed. It is evidently sufficient to multiply the numbers in this table by the value of the instrumental errors expressed in minutes of arc, in order to obtain the corrections sought for. The table is calculated for latitude 55°, near to that of Copenhagen. It is quite similar to those at present employed in all observatories. Regarding the errors of azimuth and level, the corrections differ according as we deal with a meridian transit above or below the pole, which requires a double column in the table. A note written at the side of the table indicates how it was calculated, at least that which concerns the columns relative to $D$ and $L$. Rœmer gives, in effect, the following rule: "The sine of the polar distance of the star is to the sine of its distance from the intersection as the supposed error of 1′ is to the correction sought." In order to understand what he means by "the distance of the star from the intersection" it is sufficient to refer to figures 4, 5, 6 to see that it refers to the intersection of the great circle, which the instrument describes, with the meridian. For the error $D$ it will be zenith, for the error $L$ to the north or the south point of the horizon.

If we denote by $\delta$ the declination of the star observed, and by $a$ its zenith distance the above rule, expressed algebraically, gives for the corrections:

$$\text{Azimuth} \ldots\ldots \quad D\,\frac{\sin a}{\cos \delta}$$

$$\text{Level} \ldots\ldots\ldots \quad L\,\frac{\cos a}{\cos \delta}$$

It is precisely under this form that the celebrated tables of

Tobias Mayer (1723-1762) were published in 1775. These were established to correct his observations with the mural quadrant of the Gœttingen Observatory. In reality, about 1756 he presented the results of his observations to the Gœttingen Academy ; but this work was not published until after his death. As we come to see, Rœmer preceded it here by half a century ; but the Danish Astronomer was satisfied to establish for his personal use those formulæ which all astronomers employ now under the name of *Mayer's Formulæ*.

We have said above that, historically speaking, it will be more exact to speak of the Rœmer-Talcott rather than the Horrebow-Talcott method for the determination of latitudes. Now we see also that it will be more legitimate to call by the name of Rœmer the formulæ for instrumental corrections which are ordinarily attributed to Tobias Mayer.

## CONCLUSION

These examples show how the works of Rœmer are remarkable for their epoch. The publication of his manuscript resurrected, so to speak, the figure of this man who exerted so great an influence upon the astronomical science of his time.

If the works of Horrebow had made known the greater part of the original ideas of his master, the publication of the *Adversaria* of Rœmer gives us some others. Also it is necessary to give credit to the initiative taken by the Royal Society of Sciences of Copenhagen in making this publication on the occasion of the bi-centenary of the death of one of the most illustrious of Denmark's sons.

UCCLE,
April, 1913

# THE FUNDAMENTAL PRINCIPLE OF LEAST SQUARES

By R. Meldrum Stewart

THE "Principle of Least Squares" states that the most probable values of a system of unknown quantities upon which observations have been made, are obtained by making the sum of the squares of the errors a minimum. This statement, however, is indefinite in that the term "error" is not rigorously defined; and further, the ordinary proof of the principle is defective from the fact that the same indefinite nomenclature, and loose reasoning founded thereon, are used throughout.

Let there be a system of quantities $x_1$, $x_2$, $x_3$, etc., whose true values are unknown, and suppose a set of fallible observations to have been made upon certain functions of these. The resulting observation equations will be

$$f_1(x_1, x_2 \ldots\ldots\ldots) = O$$
$$f_2(x_1, x_2 \ldots\ldots\ldots) = O'$$
etc.,

where $O$, $O'$, etc., are observed quantities. Now if $T$, $T'$, etc., are the values which $O$, $O'$, etc., would have had if the observations had been free from accidental error,* the *a priori* probability of the occurrence of the set of observations $O$, $O'$, etc., is

$$P = Ce^{-h^2(u^2 + u'^2 + \ldots\ldots)}$$

where $u = T - O$, $u' = T' - O'$, etc. As soon, however, as the observations have been made, the element of chance has disappeared, and the function $P$ is no longer a variable, but a perfectly definite, though unknown, quantity.†

* These will hereafter be referred to as the true values of the observed quantities.

† See Note at end.

The choice between two different sets of observed values, after the observations have been made, is strictly a question not of *a priori* but of *a posteriori* probability. However, we may assume that that set of observations which has the greater *a priori* probability in its favor is on the whole the nearer approach to the truth. Hence, of two sets of observed values, between which we cannot otherwise choose (*i. e.*, for which $h$ is presumably the same), that one will be, so far as can be judged, the more accurate, for which $u^2 + u'^2 + \ldots\ldots$ is the less.

Suppose now that we have two sets of adopted quantities $M$, $M'$, etc., and $M_1$, $M_1'$, etc., both close approximations to $T$, $T'$, etc., between which it is desired to choose. Since both sets of quantities are close approximations to the true values of the quantities observed, it is manifestly *possible* that they both might have resulted from actual sets of observations of equal precision; also, so far as concerns the question of accuracy of representation of the true values, it is immaterial whether they were actually observed or chosen in any other way. Hence that set of quantities is, so far as can be judged, the more accurate representation of $T$, $T'$ etc., for which $\Delta^2 + \Delta'^2 + \ldots\ldots\ldots$ is the less, where $\Delta = M - T$, $\Delta' = M' - T'$, etc. And in adopting the quantities $M$, $M'$, etc., we have assumed no restrictions except that they shall be close approximations to $T$, $T'$, etc.; hence, we may assume them to be defined in terms of adopted values of the unknowns $x_1$, $x_2$, etc., by the equations

$$f_1(x_1, x_2 \ldots\ldots\ldots) = M$$
$$f_2(x_1, x_2 \ldots\ldots\ldots) = M'$$
$$\text{etc.}$$

Hence, finally, the best values of the unknown quantities $x_1$, $x_2$, etc., are given by making $[\Delta^2]$ as small as possible (the *true* values are given by the condition $[\Delta^2] = 0$).

It is to be noted that the quantities $\Delta$, $\Delta'$, etc., are the actual errors of the quantities found by substituting any assumed values of $x_1$, $x_2$, etc., in the first members of the observation equations, and as such are distinct both from errors of observa-

tion and from "residuals". In fact, where $u$ is an error of observation and $v$ a residual, we have

$$u = T - O$$
$$v = M - O$$
$$\Delta = M - T$$

and hence $\Delta = v - u$.

If we are content, as we usually must be, to obtain the best possible approximation, not to the *true* values of the quantities observed, but to their *observed* values, the above condition (by assuming $O = T$, or $u = o$) becomes $[v^2]$ = a minimum, the ordinary statement of the law.

It must be remembered, however, that the adoption of the latter condition is simply a confession of our total ignorance of the actual errors of observation. In theoretical investigations into the Theory of Least Squares the point should be clearly in mind that there are in reality two theorems involved in the statement of the fundamental principle; (1) the condition $[v^2]$ = a minimum gives the best possible representation of the observed quantities; (2) the condition $[\Delta^2]$ = a minimum represents the true quantities which would have been given by observations free from accidental error.

NOTE.—The theorem assumed in the text-books (see, for example, Chauvenet, Vol. II., Appendix, Art. 11), that

$$P = Ce^{-h^2(v^2 + v'^2 + \ldots)}$$

is the *a posteriori* probability that the quantities $M$, $M'$, etc., (functions of adopted values of the independent unknowns $x$, $y$, etc.) are the true values of the quantities upon which observations have been made, is manifestly incorrect. The separate probabilities that $M$, $M'$, etc., are the true values of the corresponding quantities are respectively

$$p = ce^{-h^2 v^2}$$
$$p' = ce^{-h^2 v'^2}$$
, etc.

but the probability that *all* the quantities $M$, $M'$, etc., are the true values is not the product of $p$, $p'$, etc., for the reason that, after the observations have been made, $v$, $v'$, etc., (and consequently $p$, $p'$, etc.) are not independent, being connected by the system of rigorous equations

$$f_1(x_1, x_2 \ldots\ldots) = M = O + v$$
$$f_2(x_1, x_2 \ldots\ldots) = M' = O' + v'$$
$$\text{etc.}$$

In fact, the number of *independent* quantities among $v$, $v'$, etc., is the same only as the number of the independent unknowns $x_1$, $x_2$, etc.

DOMINION OBSERVATORY,
OTTAWA, CANADA,
August, 1913.

# A THEOREM IN LEAST SQUARES

By R. Meldrum Stewart

*To find a criterion to indicate whether an additional suspected unknown should be included in the solution of a set of observation equations.*

In the reduction and discussion of a series of observations it may frequently occur that the residuals appear to group themselves systematically in such a manner as to suggest the hypothesis that a solution nearer the truth might have been obtained by assuming the existence of one or more additional unknowns in the observation equations. For example, to take a simple and familiar case, the questions might arise, in a series of altitude observations by Talcott's method, (1) whether the observations were affected by error of bisection depending on magnitude; (2) whether the adopted value of one revolution of the micrometer screw was correct; (3) whether the screw was uniform throughout its length; (4) whether the adopted value of one division of the level was correct; (5) whether the level vial was uniform throughout its length; etc. Some of these questions could of course be best answered by separate investigations; but circumstances might easily occur (*e. g.* in the event of damage to a level vial or micrometer screw after the observations were completed) where this would be impossible. Many similar examples in other lines of work will readily occur to any one familiar with the making and reduction of observations.

Any such causes, if large, would in general show their effects in the residuals, if these were properly grouped; but usually such effects would not be large, and in that case it would not be evident from a simple examination of the residuals whether the apparently systematic character of the latter was fortuitous or not. It is sometimes assumed that the inclusion of an additional

unknown in the observation equations (to allow for an effect such as one of those mentioned) is justified (and the probability of the reality of the effect established) if the computed "probable error of a single observation" is thereby diminished; the computed probable error, however, represents merely an attempt to compute the precision of the *observations*, and it does not seem clear why the obtaining of a smaller value for this should necessarily involve an increased accuracy in our attempted *representation of the true quantities*; nor has any formal proof of this theorem, so far as the writer is aware, ever been attempted. It is the object of the following investigation to deduce a criterion as to whether, so far as the evidence of the observations themselves can determine, the inclusion of such an additional unknown, with the coefficients required by theory, is justified or not in any particular case.

Let there be a system of physical quantities, $n$ in number, upon each of which a single observation has been made, the values given by the observations being $O$, $O'$, $O''$, etc.; for simplicity we shall suppose the observations to be of equal weight. Denote the unknown values which would have resulted from observations free from accidental error (which we shall hereafter designate as the true values of the observed quantities) by $T$, $T'$, $T''$, etc. We shall suppose these true values to be linear functions of an undetermined number $s$ of unknown independent quantities whose true values are $X_1$, $X_2$, $X_3$, etc., where $s$ is less than $n$; so that we have the system of exact equations

$$\left.\begin{aligned} a_1 X_1 + a_2 X_2 + \ldots\ldots + a_s X_s &= T \\ a_1' X_1 + a_2' X_2 + \ldots\ldots + a_s' X_s &= T' \\ a_1'' X_1 + a_2'' X_2 + \ldots\ldots + a_s'' X_s &= T'' \\ \text{etc.,} & \end{aligned}\right\} \quad (1)$$

the quantities $a_1$, $a_2$ . . . . . , $a_1'$, $a_2'$ . . . . . etc., being considered known from theory. When we speak of the number $s$ as being undetermined, we are simply taking cognizance of the fact referred to above, that there is a possibility that one or more systematic effects, of which we are not sure, *may* have entered into the observations, in addition to the ordinary accidental

errors. Each of the quantities $N_1$, $N_2$, $N_3$, etc., is supposed to represent a physical reality (which, for example, either may or may not be a systematic error of observation), which may or may not have entered into the series of observed values; some of these we know, or have strong reasons for believing, to have actually entered; we shall suppose the number of these to be $m$; of the others, all that we require to know is that *if* they have entered, they have done so with certain co-efficients which are known from theory; the alternative that they have not entered is fully provided for by the possibility of the corresponding $N$'s having the value zero.

Now, according to one of the two theorems contained in the fundamental principle of least squares,* of any number of systems of adopted values of the unknowns, such as $x_1$, $x_2$, $x_3$, etc., that system will give the best representation of the *observations*, for which $[v^2]$ is the least, where $v$, $v'$, $v''$, etc., are determined by the equations

$$\begin{aligned} a_1\ x_1 + a_2\ x_2 + \ldots\ldots\ldots\ldots &= O + v \\ a_1'\ x_1 + a_2'\ x_2 + \ldots\ldots\ldots\ldots &= O' + v' \\ a_1''\ x_1 + a_2''\ x_2 + \ldots\ldots\ldots\ldots &= O'' + v'' \\ \text{etc.} & \end{aligned}$$

If now, according to the usual procedure, we assume the existence of the $m$ unknowns (and of these only) which we feel reasonably certain have entered into the observed quantities, we shall, by the application of the criterion $[v^2_m]$ = a minimum, obtain the ordinary least squares solution, which is the best representation of the observations attainable on the assumption of $m$ unknowns. If again, we make a similar solution on the assumption that one additional unknown has entered into the observed values (as, for example, in latitude observations, an error of bisection depending on magnitude), we shall obtain a value of $[v^2_{m+1}]$ which is necessarily less than the quantity $[v^2_m]$ obtained from the previous solution. Thus, *in every case*, the second solu-

* See "The Fundamental Principle of Least Squares," in this issue of the JOURNAL.

tion gives a better representation of the *observations* than the first, notwithstanding the fact that it is quite possible that the true value of the additional unknown may have been zero.

The problem, however, with which we are really concerned, is not whether the second solution gives the better representation of the *actually observed* quantities $O$, $O'$, $O''$, etc., but of the values $T$, $T'$, $T''$, etc., which these quantities *would have had*, had they been free from accidental error. The criterion to decide this is given by the second theorem contained in the fundamental principle,* *viz.*, that that system of adopted values is the better, for which $[\Delta]$ is the less, where $\Delta$, $\Delta'$, $\Delta''$, etc., are determined by the equations

$$\begin{aligned} a_1 x_1 + a_2 x_2 + \ldots\ldots\ldots\ldots &= T + \Delta \\ a_1' x_1 + a_2' x_2 + \ldots\ldots\ldots\ldots &= T' + \Delta' \\ a_1'' x_1 + a_2'' x_2 + \ldots\ldots\ldots\ldots &= T'' + \Delta'' \\ \text{etc.} \end{aligned}$$

Assuming (for the purpose of the least squares solution only) that the number of unknowns is $m$, and using henceforward the symbols $x_1, x_2, x_3 \ldots\ldots\ldots, v, v', v'' \ldots\ldots\ldots\ldots, \Delta, \Delta', \Delta''$ $\ldots\ldots$ to denote the values arising from the least squares solution, we shall have the following set of exact equations, $n$ in number

$$\left.\begin{aligned} a_1 x_1 + a_2 x_2 + \ldots\ldots + a_m x_m &= O + v = T + \Delta \\ a_1' x_1 + a_2' x_2 + \ldots\ldots + a_m' x_m &= O' + v' = T' + \Delta' \\ a_1'' x_1 + a_2'' x_1 + \ldots\ldots + a_m'' x_m &= O'' + v'' = T'' + \Delta'' \\ \text{etc.} \end{aligned}\right\} \quad (2)$$

where $[a_1 v] = [a_2 v] = [a_3 v] = \ldots\ldots = 0$.

Suppose the actual accidental errors of the observed quantities $O$, $O'$, $O''$, etc., to be $u$, $u'$, $u''$, etc., so that $T = O + u$, $T' = O' + u'$, $T'' = O'' + u''$, etc. Then multiplying the equations (2) by $u$, $u'$, $u''$, etc., respectively, and adding, we get

$$[a_1 u] x_1 + [a_2 u] x_2 + \ldots\ldots + [a_m u] x_m = [Tu] + [\Delta u].$$

* See "The Fundamental Principle of Least Squares," in this issue of the JOURNAL.

Similarly, from equations (1)

$$[a_1 u]\, X_1 + [a_2 u]\, X_2 + \ldots\ldots + [a_s u]\, X_s = [T u]$$

where $s > m$. Subtracting, and putting $X_1 - x_1 = \delta x_1$, $X_2 - x_2 = \delta x_2$, etc., we have

$$\left.\begin{aligned} &[a_1 u]\,\delta x_1 + [a_2 u]\,\delta x_2 + \ldots\ldots + [a_m u]\,\delta x_m + \\ &[a_{m+1} u]\, X_{m+1} + \ldots\ldots + [a_s u]\, X_s = -[\Delta u] \end{aligned}\right\} \quad (3)$$

Now $\delta x_1$, $\delta x_2 \ldots\ldots \delta x_m$ are functions of $u$, $u'$, $u''$, etc., while $X_{m+1}$, $X_{m+2} \ldots\ldots X_s$ are not. Hence the terms in (3) involving $\delta x_1$, $\delta x_2$, etc., will contain $u^2$, $u'^2$ etc., and are not negligible; the remaining terms, however, contain $u$, $u'$, etc., only to the first power, and their most probable value is therefore zero.

Thus the most probable value of $[\Delta u]$ is given by

$$-[\Delta u] = [a_1 u]\,\delta x_1 + [a_2 u]\,\delta x_2 + \ldots\ldots + [a_m u]\,\delta x_m \quad (4)$$

From equations (1) we get, putting $T = O + u$, $T' = O' + u'$, etc.,

$$\begin{aligned} &[a_1^2]\, X_1 + [a_1 a_2]\, X_2 + \ldots\ldots + [a_1 a_s]\, X_s = [a_1 O] + [a_1 u] \\ &[a_1 a_2]\, X_1 + [a_2^2]\, X_2 + \ldots\ldots + [a_2 a_s]\, X_s = [a_2 O] + [a_2 u] \\ &\qquad\vdots \qquad\qquad\qquad\qquad\qquad\vdots \qquad\qquad\vdots \\ &[a_1 a_m]\, X_1 + [a_2 a_m]\, X_2 + \ldots\ldots + [a_m a_s]\, X_s = [a_m O] + [a_m u]. \end{aligned}$$

Similarly, from (2)

$$\left.\begin{aligned} &[a_1^2]\, x_1 + [a_1 a_2]\, x_2 + \ldots\ldots + [a_1 a_m]\, x_m = [a_1 O] \\ &[a_1 a_2]\, x_1 + [a_2^2]\, x_2 + \ldots\ldots + [a_2 a_m]\, x_m = [a_2 O] \\ &\qquad\vdots \qquad\qquad\qquad\vdots \qquad\qquad\vdots \\ &[a_1 a_m]\, x_1 + [a_2 a_m]\, x_2 + \ldots\ldots + [a_m^2]\, x_m = [a_m O] \end{aligned}\right\} \quad (5)$$

Subtracting, we get

$$\left.\begin{aligned} &[a_1^2]\,\delta x_1 + [a_1 a_2]\,\delta x_2 + \ldots\ldots + [a_1 a_m]\,\delta x_m = \\ &\qquad [a_1 (u - a_{m+1} X_{m+1} - \ldots\ldots)] \\ &[a_1 a_2]\,\delta x_1 + [a_2^2]\,\delta x_2 + \ldots\ldots + [a_2 a_m]\,\delta x_m = \\ &\qquad [a_2 (u - a_{m+1} X_{m+1} - \ldots\ldots)] \\ &\qquad\qquad \text{etc.} \end{aligned}\right\} \quad (6)$$

Now equations (5) are the normal equations for the determina-

tion of $x_1$, $x_2$, etc.; also, (5) and (6) are of the same form; hence we see that if

$$\begin{aligned} x_1 &= a_1 O + a_1' O' + a_1'' O'' + \ldots\ldots \\ x_2 &= a_2 O + a_2' O' + a_2'' O'' + \ldots\ldots \\ &\text{etc.} \end{aligned}$$

we shall also have

$$\left.\begin{aligned} \delta x_1 &= a_1 (u - a_{m+1} X_{m+1} - \ldots.) + a_1' (u' - \\ &\quad a'_{m+1} X_{m+1} - \ldots.) + \ldots\ldots \\ \delta x_2 &= a_1 (u - a_{m+1} X_{m+1} - \ldots.) + a_2' (u' - \\ &\quad a'_{m+1} X_{m+1} - \ldots.) + \ldots\ldots \\ &\quad \text{etc.} \end{aligned}\right\} \quad (7)$$

Also,

$$\left.\begin{aligned} [a_1 u] &= a_1 u + a_1' u' + a_1'' u'' + \ldots\ldots\ldots \\ [a_2 u] &= a_2 u + a_2' u' + a_2'' u'' + \ldots\ldots\ldots \\ &\quad \text{etc.} \end{aligned}\right\} \quad (8)$$

Now multiplying together in order the equations (7) and (8), and noting that the most probable value of all terms not involving $u^2$, $u'^2$, etc., is zero, we obtain

$$\begin{aligned} [a_1 u]\, \delta x_1 &= a_1 a_1 u^2 + a_1' a_1' u'^2 + \ldots\ldots\ldots \\ [a_2 u]\, \delta x_2 &= a_2 a_2 u^2 + a_2' a_2' u'^2 + \ldots\ldots\ldots \\ &\quad \text{etc.} \end{aligned}$$

Putting each of the quantities $u^2$, $u'^2$, $u''^2$, etc., equal to its mean value $\epsilon^2$, and noting that*

$$\begin{aligned} a_1 a_1 + a_1' a_1' + a_1'' a_1'' + \ldots\ldots\ldots &= 1 \\ a_2 a_2 + a_2' a_2' + a_2'' a_2'' + \ldots\ldots\ldots &= 1 \\ &\quad \text{etc.} \end{aligned}$$

we have the most probable result

$$[a_1 u]\, \delta x_1 + [a_2 u]\, \delta x_2 + \ldots\ldots\ldots + [a_m u]\, \delta x_m = m \epsilon^2.$$

Hence finally, from (4)

$$[\Delta u] = - m \epsilon^2. \quad (9)$$

* For the proof of this theorem see *Chauvenet*, Vol. II., p. 515, where, however, the notation is slightly different.

From the definitions of $v$, $\Delta$ and $u$, we have

$$\begin{aligned} v &= \Delta + u \\ v' &= \Delta' + u' \\ v'' &= \Delta'' + u'' \\ &\text{etc.} \end{aligned}$$

Hence, inserting the subscript to denote the number of unknowns assumed in the least squares solution, we have

$$[v^2_m] = [\Delta^2_m] + [u^2] + 2\,[\Delta_m\, u]$$

or, substituting the value of $[\Delta_m\, u]$ from (9)

$$[\Delta^2_m] = [v^2_m] - [u^2] + 2\, m\, \epsilon^2. \qquad (10)$$

By an exactly similar process of reasoning, we have in the case of the least squares solution involving $m + 1$ unknowns

$$[\Delta^2_{m+1}] = [v^2_{m+1}] - [u^2] + 2\,(m + 1)\,\epsilon^2. \qquad (11)$$

Subtracting (11) from (10), we obtain

$$[\Delta^2_m] - [\Delta^2_{m+1}] = [v^2_m] - [v^2_{m+1}] - 2\epsilon^2. \qquad (12)$$

Now $\epsilon^2$ is unknown; if, however, the number of unknowns which were really present in the observations is $m$, its most probable value is $\dfrac{[v^2_m]}{n - m}$; in the event of that number being $m + 1$ it is $\dfrac{[v^2_{m+1}]}{n - m - 1}$; as the best approximation available we may therefore assume

$$2\,\epsilon^2 = \frac{[v^2_m]}{n - m} + \frac{[v^2_{m+1}]}{n - m - 1}.$$

Substituting this value in (12) we obtain the final equation

$$[\Delta^2_m] - [\Delta^2_{m+1}] = \frac{n - m - 1}{n - m}\,[v^2_m] - \frac{n - m}{n - m - 1}\,[v^2_{m+1}].$$

Hence the solution with $m$ or with $m + 1$ unknowns is preferable according as

$$\frac{[v^2_m]}{(n - m)^2} \gtrless \frac{[v^2_{m+1}]}{(n - m - 1)^2}$$

or as

$$\frac{r_m}{\sqrt{n-m}} \lessgtr \frac{r_{m+1}}{\sqrt{n-m-1}}$$

which constitutes the criterion required.

It is obvious that the reality of the presence of any number of suspected unknowns may be tested by repeated applications of this theorem. It must be remembered, on the other hand, that the values obtained for $[\Delta'_m]$ and $[\Delta'_{m+1}]$ are merely their most probable values, *i.e.* the average of the values which they would have if the given series of observations were repeated an indefinite number of times; and hence any inference drawn from a series consisting of a limited number of observations must be regarded only as fallible evidence. If we take the probable error of the residuals $v$, $v'$, $v''$, etc., as being equal to $r$ (the probable error of a single observed quantity), it may easily be shown that the probable error of $\sqrt{[v^2]}$ is also $r$. Hence the probable error of the quantity

$$\frac{r_m}{\sqrt{n-m}} \quad \text{is} \quad \frac{\cdot 6745}{n-m}\, r.$$

From this relation we may, in any particular case, form an idea as to the weight to be attributed to the result indicated by the criterion.

In many cases it might be possible to combine the discussion of a considerable number of groups of observations on the same or similar quantities, in all of which the same systematic effect was suspected; if the whole number of observations was large the evidence given by the criterion should then be fairly conclusive. The method of procedure in such cases would be governed by the nature of the particular observations considered, and is outside the scope of the present article.

DOMINION OBSERVATORY,
OTTAWA, CANADA
September, 1913.

## NOTES FROM THE METEOROLOGICAL SERVICE

### Summary Report of the Weather in Canada

### August, 1913

*Temperature.*—In northern British Columbia, and in parts of southwestern Saskatchewan, the mean temperature for the month was slightly below normal, while in the southern portion of the former province, on Vancouver Island, and in all other parts of the West, the month was warmer than average, the positive departure being from 3° to 4° in the southern districts of Manitoba. Over nearly the whole of southern Ontario the average temperature for the month varied from normal to 2° above normal, but locally in the Lake Huron district there was a negative departure. Nearly everywhere in Quebec and the Maritime Provinces temperatures for the month were slightly below average, small positive departures being recorded, however, in Cape Breton and locally on the Lower St. Lawrence.

*Precipitation.*— The rainfall during the month was greatly in excess of average throughout the Prairie Provinces, the positive departure reaching as high as 140 per cent. in parts of southwestern Saskatchewan. In Ontario the drought of July continued on into August, but was partially broken on the 9th, when rain occurred which in some places was quite heavy. This was again followed by a dry spell, lasting until the 22nd, when a general and heavy rain fell; and rains were then recorded at frequent intervals until the close of the month. In Quebec and the Maritime Provinces there was everywhere a great deficiency

of rain, the extreme negative departure being in southwestern Nova Scotia, where only about one-sixth of the normal fall occurred.

### September, 1913

*Temperature.*—In southern British Columbia, in Vancouver Island, and in southeastern Saskatchewan the mean temperature for the month was slightly below average, but in all other parts of the West the month was warmer than usual, the differences reaching from 3° to 5° above normal in southern Alberta and western Saskatchewan. Over nearly the whole of Ontario the month was from 2° to 4° cooler than average, but locally in the Lake Huron region a slight positive departure was recorded. In southwestern Quebec and in the Maritime Provinces temperatures for the month were from 2° to 3° below normal, while in the Lower St. Lawrence Valley there was a slight positive departure.

*Precipitation.*—The rainfall for the month was deficient throughout the greater part of the Dominion. Near the western end of Lake Superior, and also in counties contiguous to the Georgian Bay, the Lower Ottawa Valley, and the St. Lawrence Valley, from Montreal to a little east of Quebec, a greater quantity than usual was recorded, this being also the case on Cape Breton Island.

## Temperatures for August and September, 1913

| Station | August | | September | |
|---|---|---|---|---|
| | Highest | Lowest | Highest | Lowest |
| *Yukon* | | | | |
| Dawson | 80 | 23 | 68 | 22 |
| *British Columbia* | | | | |
| Atlin | 68 | 28 | 56 | 28 |
| Agassiz | 91 | 49 | 78 | 43 |
| Barkerville | 78 | 32 | 66 | 28 |
| Kamloops | 95 | 47 | 82 | 36 |
| New Westminster | 90 | 48 | | |
| Prince Rupert | 78 | 46 | 70 | 43 |
| Vancouver | 82 | 48 | 71 | 41 |
| Victoria | 86 | 46 | 71 | 41 |
| *Western Provinces* | | | | |
| Battleford | 85 | 40 | 88 | 22 |
| Broadview | | | | |
| Calgary | 90 | 38 | 84 | 30 |
| Carman | | | | |
| Edmonton | 83 | 38 | 78 | 27 |
| Medicine Hat | 97 | 44 | 88 | 30 |
| Minnedosa | 89 | 43 | 88 | 23 |
| Moose Jaw | 91 | 45 | 89 | 24 |
| Oakbank | 88 | 42 | 81 | 21 |
| Portage la Prairie | 92 | 45 | 79 | 24 |
| Prince Albert | 82 | 38 | 84 | 29 |
| Qu'Appelle | 85 | 40 | 86 | 27 |
| Regina | 85 | 42 | | |
| Swift Current | 94 | 39 | 90 | 20 |
| Winnipeg | 91 | 44 | 83 | 23 |
| *Ontario* | | | | |
| Agincourt | 96 | 49 | 88 | 32 |
| Aurora | 92 | 35 | 86 | 28 |
| Bancroft | 93 | 31 | 86 | 23 |
| Barrie | | | 86 | 34 |
| Beatrice | 88 | 54 | 85 | 25 |
| Birnam | 87 | 41 | 88 | 35 |
| Bloomfield | 98 | 41 | 85 | 32 |
| Brantford | 95 | 45 | 89 | 29 |
| Bruce Mines | 88 | 38 | 81 | 26 |
| Chatham | 96 | 50 | 97 | 36 |
| Clinton | 93 | 37 | 89 | 31 |
| Cottam | 96 | 39 | | |
| Gravenhurst | 92 | 31 | 88 | 21 |
| Guelph | 95 | 38 | | |
| Haliburton | 94 | 31 | 90 | 25 |
| Hamilton | 93 | 41 | 87 | 36 |
| Huntsville | 93 | 32 | 89 | 25 |
| Kenora | 87 | 29 | 73 | 29 |
| Kinmount | 90 | 31 | 84 | 21 |
| Kingston | 88 | 41 | 80 | 35 |
| Lakefield | 91 | 44 | | |
| London | 94 | 38 | 91 | 33 |
| Lucknow | 89 | 35 | 90 | 30 |
| Madoc | 94 | 37 | 85 | 27 |
| Midland | 91 | 54 | 92 | 40 |
| North Gower | 95 | 35 | 88 | 27 |
| Otonabee | | | | |
| Ottawa | 92 | 41 | 85 | 35 |
| Owen Sound | | | | |
| Paris | 94 | 42 | 89 | 31 |
| Parry Sound | 92 | 37 | 87 | 30 |
| Peterboro' | 97 | 39 | 87 | 26 |
| Port Arthur | 85 | 38 | 77 | 29 |
| Port Burwell | 84 | 41 | 81 | 31 |
| Port Dover | 88 | 42 | 87 | 34 |
| Port Stanley | 86 | 41 | 80 | 33 |
| Renville | 93 | 33 | 86 | 29 |
| Sarnia | | | | |
| Southampton | 89 | 41 | 92 | 36 |
| Stonecliffe | 97 | 36 | 88 | 28 |
| Stony Creek | 95 | 44 | 89 | 34 |
| Stratford | 91 | 36 | 87 | 28 |
| Toronto | 92 | 42 | 85 | 36 |
| Uxbridge | 96 | 37 | 85 | 29 |
| Wallaceburg | 91 | 42 | 92 | 32 |
| Welland | 91 | 45 | 88 | 35 |
| White River | 86 | 27 | 79 | 15 |
| *Quebec* | | | | |
| Brome | 87 | 34 | 83 | 24 |
| Father Point | 75 | 39 | 73 | 33 |
| Montreal | 89 | 43 | 81 | 34 |
| Quebec | 83 | 40 | 84 | 29 |
| Sherbrooke | 88 | 38 | | |
| *Maritime Provinces* | | | | |
| Charlottetown | 81 | 43 | 79 | 36 |
| Chatham | 85 | 40 | 83 | 31 |
| Dalhousie | 82 | 41 | 80 | 32 |
| Fredericton | 86 | 36 | 85 | 29 |
| Halifax | 87 | 39 | 85 | 35 |
| Moncton | | | | |
| St. John | 77 | 47 | 70 | 36 |
| St. Stephen | | | 90 | 27 |
| Sussex | 83 | 34 | 80 | 31 |
| Sydney | 83 | 41 | 81 | 30 |
| Yarmouth | 74 | 41 | 75 | 31 |

## Earthquake Records by the Milne Seismograph

### TORONTO

R. F. Stupart, Director.

P.T. = Preliminary Tremors. L.W. = Large Waves. A.C. = Air Currents.

Time is Greenwich Civil Mean Time, 0 or 24 h = midnight.

| No. | Date 1913 | | P. T. Comm. | L.W. Comm. | Max. | * End | Max. Amp. | Durat. | Remarks |
|---|---|---|---|---|---|---|---|---|---|
| | | | h m | h m | h m | h m | mm | h m | |
| 1271 | July | 26 | 21 4·0 *f*<br>21 9·2 *s* | | 21 14·5 | A. C. | 0·3 | | A.C. going on. |
| 1272 | " | 28 | 5 45·0 | 5 57·7<br>6 2·0 | 6 18·7 | 7 22·7 *f*<br>7 45·0 *s* | 0·3 | 1 57·9 | |
| 1273 | " | 28 | 13 20·4 | | | 14 23·7 | 0·1 | 3·3 | A. C. going on. |
| 1274 | " | 29 | 15 57·6 | 16 19·7 | | 16 49·5 | 0·1 | 1 51·9 | |
| 1275 | Aug. | 1 | 17 6·4 *f*<br>17 20·9 *s*<br>17 33·2 *s* | 17 41·4 | 17 42·9 | A. C. | 0·8 | | Beginning doubtful. |
| 1276 | " | 5 | 1 53·4 *f*<br>2 01·6 *s* | 21 13·9 | 2 20·0 | 3 2·0 | 0·8 | 1 8·6 | Beginning doubtful. |
| 1277 | " | 6 | 22 20·7 *f*<br>22 24·0 *s* | 22 32·8 | 22 33·7 | 3 27·7 | 4·3 | 5 07·0 | Perx. rapid vibrations, 22h 42·6m to 22h 57·0m. |
| 1278 | " | 7 | 2 33·9 | | | 3 38·9 | 0·05 | 1 5·0 | |
| 1279 | " | 13 | | 5 06·0 | | Air Currents | | | A. C. going on. |
| 1280 | " | 13 | 5 41·2 | | | 6 35·8 | 1·05 | 1 51·6 | A. C. going on. |
| 1281 | " | 15 | 19 21·6 *f*<br>19 52·4 *s* | | | 20 42·9 | 0·05 | 1 21·3 | A. C. going on. |
| 1282 | " | 31 | 7 9·0 | | | 8 0·0 | 0·1 | 0 51·0 | |
| 1283 | " | 31 | 18 11·0 | | | 18 42·0 | 0·05 | 0 31·0 | |
| 1284 | Sept. | 3 | 21 17·4 *f*<br>21 19·1 *s* | 21 49·5 | 21 51·8 | 23 15·4 | 0·3 | 1 58·0 | No defined max. |
| 1285 | " | 15 | 5 51·3 *f*<br>5 59·2 *s* | 6 15·5 | | 6 31·1 | 0·05 | 0 39·8 | |
| 1286 | " | 23 | 11 11·9 *f*<br>11 18·9 *s* | | | | 0·05 | | A. C. going on. |
| 1287 | " | 26 | 9 09·3 | | | 9 53·9 | 0·3 | 0 40·7 | Began suddenly. |
| 1288 | " | 30 | 4 15·9 | 4 25·9 | | 4 48·2 *f*<br>5 04·0 *s* | 0·05 | 0 48·1 | Suspicion of A. C. |

Period 17·7 seconds. 1 mm. = 0″·59.

## VICTORIA, B. C.

E. B. REED, SUPERINTENDENT.

| No. | Date 1913 | P.T. Comm. | I.W. Comm. | Max. | End | Max. Amp. | Durat. | Remarks |
|---|---|---|---|---|---|---|---|---|
| | | h m | h m | h m | h m | mm. | h m | |
| 1276 | July 26 | 21 18·0 | 21 20·2 | | 21 29·4 | 0·2 | 0 11·4 | |
| 1277 | " 28 | 5 52·1 | 6 1·1 / 6 24·9 \ | | 7 35·6 | 0·2 | 1 43·5 | A. C. going on. |
| 1278 | " 28 | 13 16·6 | | | 14 25·5 | 0·1 | 1 18·9 | |
| 1279 | " 29 | 15 50·8 | | | 16 8·7 | 0·05 | 0 17·9 | |
| 1280 | Aug. 1 | No P.Ts. | 17 27·0 | | 19 7·0 | 0·2 | 1 40·0 | Max. not defined. |
| 1281 | " 5 | 1 53·4 | 1 57·0 | 2 1·3 | 2 31·1 | 0·3 | 0 37·7 | |
| 1282 | " 6 | 22 27·2 | 22 41·1 | 23 3·4 | 2 03·02 | 7·2 | 3 41·8 | Peru--- Amplitude of first and second phase larger than usual. |
| 1283 | " 7 | 2 32·22 | | | 3 30·0 | 0·3 | 0 57·8 | |
| 1284 | " 13 | 5 48·6 | | | 6 02·5 | 0·05 | 0 13·9 | |
| 1285 | " 15 | 19 52·7 | | | Attending to inst. | 0·05 | | |
| 1286 | " 31 | 6 55·7 | | | 7 50·9 | 0·2 | 0 55·2 | |
| 1287 | " 31 | 17 58·02 / 17 59·9 \ | | | 18 25·0 | 0·05 | 0 26·1 | |
| 1288 | Sept. 3 | 21 8·4 | 21 14·4 / 21 32·4 \ | 21 58·4 | 23 48·0 | 0·8 | 2 39·6 | Marked dis. |
| 1289 | " 15 | 5 57·4 | | | 6 1·6 | 0·05 | 0 4·2 | |
| 1290 | " 26 | 9 37·4 | 9 28·2 | 9 28·8 | 9 35·1 | 0·6 | 0 07·7 | Boom came gradually to rest. |

Period 18 seconds. 1 mm. = 0".54

J. V.

## MAGNETIC OBSERVATIONS

At Agincourt during the months of August and September the only disturbance of note in the magnetic force was recorded on the 8th and 9th of September and it was more effective in producing a change of direction rather than a change of force in the earth's field. On the Declination Curve it appeared as a series of long waves whose maximum amplitude was 46′·1, whilst in Horizontal Force the maximum amplitude only reached 0·00091 dynes.

On 19 days in August and on 17 days in September the magnetic curves were classed as normal.

Declination

| Month | Mean of Month | Max. | Date | Min. | Date | Monthly Range | Amplitudes—Mean Daily From hourly readings | Amplitudes—Mean Daily From Means of Extremes |
|---|---|---|---|---|---|---|---|---|
| 1913 | ° ′ | ° ′ | | ° ′ | | ° ′ | ′ | ′ |
| Aug. | 6 19·4 | 6 29·0 | 15 | 6 03·4 | 12 | 0 25·6 | 12·5 | 14·7 |
| Sept. | 6 19·6 | 6 46·4 | 8 | 6 00·3 | 8 | 0 46·1 | 10·7 | 15·6 |

Horizontal Force—C.G.S. Units

| | | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| 1913 | | | | | | | | |
| Aug. | 0·16127 | 0·16178 | 11 | 0·16080 | 16 | 0·00098 | 0·00038 | 0·00053 |
| Sept. | 0·16109 | 0·16140 | 22 | 0·16058 | 3 | 0·00091 | 0·00037 | 0·00053 |

Inclination

| Month | Mean Earth Inductor |
|---|---|
| 1913 | ° ′ |
| August | 74 40·8 |
| September | 74 41·3 |

## ASTRONOMICAL NOTES

CLASSIFICATION OF STELLAR SPECTRA.—At the meeting of the Royal Astronomical Society, June 13th, Professor E. C. Pickering gave an account of some of the work in progress at the Harvard University. He specially mentioned the great work of Miss Cannon in the classification of stellar spectra. In three years she had classified 1,000 spectra, but by careful organisation Miss Cannon and a staff of assistants now classified 5,000 spectra in a month. The number to be dealt with was enormous; it would, perhaps, amount to 150,000, or even 200,000. Professor Pickering spoke also of the work on short-period variables, and mentioned one star which doubled its light in seven minutes. Miss Cannon also spoke, mentioning that in classifying the spectra of gaseous nebulæ she found precisely the character of the spectra of 5th star type stars. — *The Journal of The British Astronomical Association*, No. 8.

HENRI POINCARÉ, HIS SCIENTIFIC WORK AND PHILOSOPHY. — The last century has produced experimenters of genius like Pasteur — men of astonishing intuition like Maxwell. It has not produced men who have done as much as Poincaré for the progress of the purely deductive sciences and for mathematical discipline, or who like him could "think science" and place it exactly. The picture which he has left us is at the same time sad and encouraging. Science has its limits. It can know only the relative, but in that it is supreme. As to wishing to penetrate into what is called the absolute — the "things in themselves" — these questions are not only insoluble but illusory and

void of sense. Science is an asymptote to the total truth as is the hyperbola asymptote to its directrices, and further, like the hyperbola, it extends without end.

In the somber forest of mystery, learning is like a glade. Men enlarge continuously the circle which borders the clearing. But at the same time it continuously touches the shades of the unknown at a greater number of points. No one on the borders of this glade has known how to gather newer and more magnificent flowers than has Henri Poincaré. So, as long as there are men who think that it is noble to live at the summit where harsh truth is enthroned, his Loraine name will tremble on their lips. —CHARLES NORDMANN, in *Publications* Smithsonian Institution, No. 2226.

SPIRAL NEBULÆ.— We needs must ask of what are the spiral nebulæ formed; in what way are they changing? Could we answer these two questions, then we would ask two others still more ambitious. How were the spiral nebulæ formed, and what will be their end? But such questions may for a long while yet be premature, and I believe I thus voice the opinion of our master, Poincaré, if I rightly interpret the conclusions stated in his recent book on cosmic hypotheses.

It seems to me that the elements of the spiral nebulæ can be nought else than collections of groups of stars, whence comes the abundance of the luminous points scattered in the outer portions of the spirals where they can be separately seen.

I believe that we should not derive from our latest studies the theories of Chamberlain and Moulton or of Professor Arrhenius, all three of whom interpret the spirals as due to a collision of two stars. Mr. T. J. J. See makes a spiral have its birth in the meeting of two clouds of very elongated form which move through space with different velocities and become deformed before uniting under the influence of their mutual attraction. Each spiral marks the influx towards the common center of one of the original clouds.

I fear that such an explanation would be satisfactory only to readers but little acquainted with the objects themselves. It is

not merely two concurrent spirals which we must explain, but often four or five. And when we consider the parsimonious scattering of matter through space, it is truly difficult to admit that upon the path of the deflecting current there will appear first isolated stars, then clusters of stars more and more numerous and more and more dense as we approach the place of conjunction. I do not see whence will be gathered the matter for these suns if there is no central condensation, which, according to See, would not yet have been formed. To me the movement in spirals must be centrifugal and dispersive. The central mass shoots out intermittently groups of stars, giving them a great initial velocity, but the impulsive force acts only for a short distance. The final movement of the liberated stars is governed by the general attraction, except in the neighborhood of certain points of the spirals which have in turn become centres of disturbances.—PROFESSOR P. PUISEUX, of the Paris Observatory, *Publications* Smithsonian Institution, No. 2190.

THE CLUSTER VARIABLE RR LYRÆ.—In a letter to Director Campbell, Hertzsprung, of Potsdam, called attention to the fact that RR Lyræ is the brightest Cluster Variable so far known. Its light ranges between the seventh and eighth magnitude, thus enabling it to be observed with a low-dispersion spectrograph attached to the 36-inch refractor. As a result of his own observations Hertzsprung has found a period of 0·56682 day.

The relations between the light and velocity variations of RR Lyræ leave but little doubt that the Cluster Variables are only a subdivision of the great family of short-period variables. Hence, any theory put forth in explanation of the Cepheid-Geminid type of variation must be broad enough in its scope to include the Cluster Variables also. The most plausible theories thus far advanced find a cause for the light variations of the short-period stars in a resisting medium in which they are supposed to be enveloped. The idea, first suggested by Curtiss and later developed by Loud, presumed the medium to consist of meteoric particles which, as a result of collisions with the bright star, brought its advancing side to a higher state of incandescence

than its following side. Obvious objections led to the rejection of the meteoric theory by Duncan, who proposed in its stead one which attributes the star's variation to changes in the absorbing powers of its atmosphere. According to this theory the atmosphere of the bright star is continuously brushed back by the material in a nebulous envelope, in which it moves, surrounding its companion, the envelope in all liklihood being a corona such as that surrounding our own sun. As the star approaches the observer the light emanating from it passes through a much smaller depth of atmosphere than when the star is receding. Consequently the light from the advancing side is not so strongly absorbed as that from the following side.

This theory accounts in an admirable manner for the one characteristic that is so distinctively a feature of short-period variables; namely, that maximum light occurs at the time of maximum velocity of approach. It has, therefore, been made the basis of a theory which Roberts has advanced in explanation of the variation of S Aræ, a well-known member of the family of Cluster Variables. According to this theory the Cluster Variables are binary systems in which the bright star varies in brilliancy according to the hypothesis of Duncan; but the plane of the star's orbit lies in the plane of sight so that periodically the phenomenon of eclipse occurs, which is represented by the constant phase in the star's light curve.

The results of the investigations of RR Lyræ, and those of the Cepheid-Geminid stars already studied, are such as to indicate that the non-eclipsing variables of short period may all be considered as members of one class. Before a satisfactory theory can be advanced in explanation of the short-period type of variation, we must know more of the nature of the light-variation as observed in different wave-lengths, and of the relationships existing between such variations and the orbital motions of the systems.—C. C. Kiess, Lick Observatory *Bulletin*, No. 232.

J. R. C.

## NOTES AND QUERIES

**Communications are Invited, Especially from Amateurs. The Editor will try to Secure Answers to Queries.**

### JUPITER IN A THREE-INCH TELESCOPE

From the Harvard Astronomical Station, Mandeville, Jamaica, W. I., Professor W. H. Pickering writes:—

"A correspondent asks if the shadows of Jupiter's satellites are visible with a 3-inch telescope. With a magnification of not less than 80 diameters the shadows are readily visible with a 3-inch aperture, or even less, under favorable conditions. The shadow of the third satellite is particularly easy. Transits of the fourth, the dark satellite, should also be seen."

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### STELLAR EVOLUTION

The problem of the development of the numerous suns of space is one of the greatest ever attempted by the human mind. Noted progress has been made towards its solution during recent years, seen especially in the demonstration of a close relation between spectral class, color index (or the difference between the visual and the photographic magnitude) and radial velocity.

A very clear and concise statement of the present status of the problem appears in No. 149 of the *Publications* of the Astronomical Society of the Pacific, the author being Miss E. Phœbe Waterman, Ph.D., who was recently appointed to a position in the Argentine National Observatory of which Professor C. D. Perrine is director.

After a full discussion of the various types of spectra and their classification by different astronomers, and an account

of laboratory investigations made with the object of elucidating the questions presented by the stellar spectra, Miss Waterman concludes that the Harvard classification is at present the most satisfactory. The connection between spectral type and radial velocity, demonstrated chiefly by Campbell, is certainly strong evidence for this order of development. The following table gives in brief the Harvard order, together with such of the arguments for it as can conveniently be tabulated. The headings are self-explanatory.

| Class | Spectrum | Average Velocity | Binaries |
|---|---|---|---|
| *O* | Bright bands.<br>*H* and *He* present. | | Very few visual binaries. One in three are spectrocopic binaries, with periods generally short and eccentricities small. |
| *B* | Absorption lines.<br>*He* at maximum.<br>*H* strong.<br>*Ca* (*K*) and *Mg* present | 6·2 km. | |
| *A* | Absorption lines.<br>*H* at maximum.<br>*Ca* (*K*) and *Mg* stronger.<br>Enhanced lines of metals present. | 10·5 km. | Numerous visual binaries. About one in six are spectroscopic binaries; periods longer than Classes *O* and *B*. |
| *F* | Absorption lines.<br>*H* weaker.<br>*Ca* (*K*) stronger.<br>Enhanced lines strong.<br>Arc lines present. | 14·4 km. | |
| *G* | Absorption lines.<br>*Ca* (*K* and *K*) stronger.<br>Enhanced lines weaker.<br>Arclines strong. | 15·0 km. | Numerous visual binaries. About one in six are spectroscopic binaries; periods generally long. |
| *K* | Absorption lines.<br>*Ca* (*H* and *K*) very strong.<br>Enhanced lines very weak. | 16·8 km. | |

| Class | Spectrum | Average Velocity | Binaries |
|---|---|---|---|
| *M* | Absorption lines. Absorption of short wave-lengths strong. Bands present, due to chemical compounds. | 17·1 km. | Very few binaries either visual or spectroscopic. |
| *N* | Absorption lines. Absorption of short wave-lengths strong. Bands present, due to carbon. | .... | Unknown. |

## THE METEORIC DISPLAY OF FEBRUARY 9.

The present writer has received many communications referring to his paper on the above phenomenon, two of the most interesting being from Professor W. H. Pickering, of the Harvard Observatory, and Mr. W. F. Denning, of Bristol, England.

Professor Pickering writes :—"What a pity it [the meteoric display] was seen by no skilled observers, *i. e.*, astronomers! Something analogous, I presume, occurred at the time of the formation of the Meteor Crater in Arizona, the date of which was, perhaps, in August, 1029, when it is recorded at Cairo 'many stars passed with a great noise and brilliant light.' Cairo is, however, 100° distant from Arizona, which seems pretty far.

The great shower of Leonids in 1901 was not seen to any great extent north of latitude + 30°, and apparently not far to the south of it, so that it too was limited to a rather narrow path."

Mr. Denning wrote that he considered the display to be without a parallel. He also said that he had looked into the data contained in my paper and considered the height assigned by me to be too small. At my request, Mr. Denning has prepared a paper dealing with the phenomenon. It is an interesting and valuable discussion of the circumstances of the display and will appear in the next issue of the JOURNAL.

## THE PRESENT CONDITION OF SOME OF THE TEMPORARY STARS

Few celestial objects evoke more interest than the *novæ* or new stars which come into prominence with such dramatic suddenness and which usually fade away after a brief existence. In the *Astronomische Nachrichten*, No. 4655, May, 1913, Professor E. E. Barnard, of the Yerkes Observatory, gives the latest information regarding some of these bodies. From this paper the following facts are extracted. The co-ordinates given are for 1900.

*Nova T Coronæ Bor.* (1866). Birmingham's star. $\alpha$ 15$^h$ 55$^m$ 19$^s$·3, $\delta$ + 26° 12′·2. In May, 1907, the star was 9·4 magnitude and apparently colorless, quite like an ordinary star.

*Nova Cygni* (1876). Schmidt's star. $\alpha$ 21$^h$ 37$^m$ 47$^s$·2, $\delta$ + 40° 23′·1. On October 29, 1911, about magnitude 15. Its appearance is distinctly hazy and it requires a longer focus than an ordinary star. It is probably slightly variable in its light.

*Nova Andromedæ* (1885). Hartwig's star. $\alpha$ 0$^h$ 37$^m$ 15$^s$·1, $\delta$ + 40° 43′·2. Professor Barnard has looked for this object a number of times in the past twenty years with both the 36-inch telescope of the Lick Observatory and the 40-inch of the Yerkes Observatory, but nothing has been seen of it. On November 15, 1912, under good conditions nothing could be seen of it with the 40-inch.

*Nova Aurigæ* (1891). Anderson's star. $\alpha$ 5$^h$ 25$^m$ 34$^s$·0, $\delta$ + 30° 22′·2. On October 22, 1901, it was of the 13th magnitude, and on November 16, 1912, of the 14th magnitude. Its image is ill-defined and it seems to require a longer focus than an ordinary star.

*Nova Sagittarii* (1898). Mrs. Fleming's star. $\alpha$ 18$^h$ 56$^m$ 12$^s$·6, $\delta$ − 13° 18′·2. In recent years it has been slowly fading. It is always hazy and ill-defined. On July 16, 1912, it was of the 15th magnitude.

*Nova Persei* (1901). Anderson's star. $\alpha$ 3$^h$ 24$^m$ 24$^s$·1, $\delta$ + 43° 33′·7. At present the star is colorless, quite like an ordin-

ary star. On September 8 and October 15, 1912, the magnitudes 12·1 and 12·0 respectively.

*Nova Geminorum* (1903). Turner's star. $\alpha$ 6h 37m 49s·0, $\delta$ + 30° 2′·7. On October 15, 1912, of magnitude 16. From present indications the star will, perhaps, eventually pass out of reach of the 40-inch telescope.

*Nova Aquilæ* (1905). Mrs. Fleming's star. $\alpha$ 18h 56m 49s·0, $\delta$ − 4° 35′·3. On July 13, 1912, under good conditions no trace of it could be seen. It has passed beyond the reach of the 40-inch telescope and must now be less than the 17th magnitude.

*Nova Lacertæ* (1910). Espin's star. $\alpha$ 22h 31m 45s·1, $\delta$ + 52° 11′·9. On December 31, 1912, its magnitude was 12·5. It then presented the appearance of a very small nebula, less than 2″ in diameter, of a bluish white color. The focus was 5 mm. greater than for an ordinary star, and when it was in best focus it was still very ill-defined.

*Nova Geminorum*, No. 2, (1912). Enebo's star. $\alpha$ 6h 48m 25s·0, $\delta$ + 32° 16′·0. Fluctuating now in brightness. On October 29, 1912, magnitude 7·7; November 10, 8·2; November 17, 7·5; January 12, 1913, 8·3.

*Nova Cassiopeiæ* (1572). Tycho Brahe's star. $\alpha$ 0h 19m 0s·2, $\delta$ + 63° 34′·7. There are several faint stars near this position, none of which can certainly be identified as Tycho's nova.

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## THE RULER OF THE WEATHER

It will be a long time before the moon is dethroned from its position in popular estimation as the ruler of our weather. During the third week in August the writer was in a barber's "parlor" in a thriving country town. The weather had been exceptionally dry and hot for a long period, forest fires were raging all about and vegetation was suffering severely. Of course a remark was made about the weather and some anxiety expressed that the drought would soon be broken. The workman immediately walked to the other end of the room and con-

sulted a calendar, and on returning remarked that the moon was full on the 16th and that the next change would be on the 24th. He was sure there would be no change in the weather until then! I tried to analyse the reasons for his faith, but no argument could change his belief.

Recently I have been reading an old book which deals with this and other superstitions. It is entitled "Miscellaneous Reflections occasioned by the Comet which appeared in December, 1680." The author is Pierre Bayle and the book, written in French, was published in 1682, an excellent English translation appearing in 1708. Bayle's arguments are always neatly stated and are accompanied with witty illustrations. I venture to quote from Section 46 which deals with the moon and the weather:—

> If the influences of any of the heavenly bodies have a secret virtue with respect to the earth, those of the moon undoubtedly have, because she is the nearest. Accordingly people are persuaded she has an influence over a great variety of things. She it is that causes the increase and decrease of the marrow and brain in animals, that frets away stones, that governs the cold and the heat, the rains and the winds. For if the weather be set in for rain at the new moon, you must not expect fair weather till the full. If the rain does not cease then, you're to reckon it will hold till the next change; and so of dry and frosty weather, etc., upon a principle it belongs to the conjunctions and oppositions of the moon to order the weather and the season. Hence it is, that because in common conversation we frequently fall upon a discourse of the rain, or the cold, or dry weather, etc., we hear those who complain of the weather comfort themselves between whiles with the hopes of the new or full moon, which as they imagine will cause a change. You can't deny, sir, these are opinions of all countries, and common to all sorts of people.
>
> Nevertheless, they who have taken the trouble for twenty or thirty years together to examine into marrow-bones, have observed, that whatever quarter the moon was in, they met with some bones which had a great deal of marrow in 'em, and others that had very little; which shows that the moon has no influence on the substances of the brain or marrow, nor yet on the substance of the fish in oysters and lobsters; for by making their observation in this point too, they have discovered that their fulness depends not more or less upon the time of the moon, whatever the popular opinion would persuade us. I say the same of the change of weather, and maintain from my own experience, 'tis not appointed to any season of the moon whatsoever, and that there's no certain day in the lunar month on which the change, for example, from wet to dry, from open to frosty weather,

happens oftener than on any other day besides. Did we make exact observations, we should find the temperature of the air has so little sympathy with the new or full moon, that we might count as many months of dry or wet weather, when the return of the moon was wet, and contrariwise; so true it is, that the changes of the weather are subject to no rule obvious to us. 'Twere easy to show, that the reason of the thing is directly against the common opinion: but I choose to argue from experience, and insist, that whoever appeals to this argument, will find it contradicts the general opinion of the world. And upon this I must take leave to observe, there's nothing strange in an error's becoming universal, considering the little care men take to consult their reason, when they give in to what they hear others say, and the little use made of the occasions offered for undeceiving 'em.

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## A NEW EDITION OF AN IMPORTANT WORK

It is always a pleasure to see a good book reach a new edition, especially if it is one difficult to write, expensive to print and appealing to a limited constituency. Such is the case with Professor J. P. C. Southall's "Principles and Methods of Geometrical Optics," (Macmillan, N. Y., price $5.50). The first edition, published in 1910, was reviewed in this JOURNAL, (Vol. IV. p. 407, 1910); and it is creditable, both to the author and to the scientific public, that a second edition has been demanded in such a short time.

In addition to correcting a number of slips, inevitable in a work so extensive and so full of mathematical symbols, the author has re-written several sections and has supplied some omissions. A fifteen-page appendix to chapter XI., contain a fuller discussion of some questions arising in the consideration of the refraction of an infinitely narrow bundle of rays through an optical system; while eleven pages are added to chapter XII., which deals with the theory of aberrations. The work is certainly an admirable modern treatise on geometrical optics, and it is hoped that it is assisting English speaking people to regain a portion of the ground they lost to their German brethren.

I understand Professor Southall is planning to bring out a series of volumes on modern optical instruments, the introductory volume to deal with the eye and spectacles. Judging from

the splendid work just noticed it can be predicted that the new volumes will be of the highest value, and will take their place as standards in the subjects treated.

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## DEATH OF "KELVIN McKREADY"

A short time ago "A Beginner's Star-Book" was noticed in these columns and highly commended. The author was given as Kelvin McKready, but from *Popular Astronomy* I learn that that was simply a pen name, the real name being Edgar Gardner Murphy and that he died recently. He was born in Arkansas, August 31st, 1869. For twelve years he was an Episcopal clergyman, but in 1903 he retired in order to devote himself to the improvement of educational and civic conditions, especially in the Southern States. His two books "Problems of the Present South" and "The Basis of Acendency" are well known; and in spite of protracted physical weakness his was a very active life. He was organiser and first secretary of the National Child Labor Committee, executive secretary of the Southern Educational Board, and vice-president of the Conference for Education in the South. His introduction to astronomy is certainly a masterpiece of its kind.

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## IS ZERO MAGNITUDE HIGH OR LOW?

The following sentence, referring to celestial globes, is from a letter received from a dealer in scientific apparatus: "The maps are exceedingly accurate and on the 12-inch size all stars are shown including those of the eighth magnitude while the 18-inch size includes all of the sixth and the ecliptic circle is graduated in degrees."

This reminds me of the political orator who, in a vigorous attack on 'the government,' exclaimed: "They tax us a *tenth*, they'd take a *twentieth* if they dared!"

C. A. C.